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PSYCHOLOGICAL REVIEW PUBLICATIONS

THE
Psychological Monographs

EDITED BY

JAMES ROWLAND ANGELL, UNIVERSITY OF CHICAGO
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Index*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*Review*) and
SHEPHERD I. FRANZ, GOVT. HOSP. FOR INSANE (*Bulletin*)

VOLUME XVII

1914

137156
26/11/15

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.);
LEIPZIG (Koenig Str., 37); PARIS (16, Rue de Condé)

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(1)

Vol. XVII
No. 1

PSYCHOLOGICAL REVIEW PUBLICATIONS

August, 1914
Whole No. 72

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF HARVARD UNIVERSITY

An Experimental Study of Decision Types and Their Mental Correlates

By
JAMES WINFRED BRIDGES

PSYCHOLOGICAL REVIEW COMPANY
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LEIPZIG (Hospital St., 10); PARIS (76 rue de Rennes)

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AN EXPERIMENTAL STUDY OF DECISION TYPES AND THEIR MENTAL CORRELATES

It is a matter of common observation that individuals differ greatly in decision *time, accuracy,* and *constancy* of or consistency in subjective decisions. It is generally supposed that the slow in decision are both more constant and more accurate than the quick. "Slow but sure" is a popular maxim. The chief purpose of this investigation was to determine the actual nature of these various decision types—to see, for example, whether "quick but sure" and "slow but unreliable" are not equally true of certain classes. In other words, it was desired to ascertain the relation between decision *time* and *constancy,* between *time* and *accuracy,* and also between the subjective and objective decision types thus determined. A further purpose was to determine the correlation subsisting between these decision factors (viz.: time accuracy and constancy) and other mental traits such as memory, association time, suggestibility, attention, etc.

The experiments were conducted in the Harvard Laboratory during the years 1911 to 1913. In all, nineteen subjects took part; three of them (A, C and L) continued through the two years' work. Twelve participated in the first year's experiments reported in the first part of this paper, and ten in the second year's experiments reported in Chapter II. All were members of the laboratory and trained in experimental methods. Five (D, F, I, O, and P) were women. Three of these were in the first year's investigation and two in the second.

In the first year's experiments on decision the materials used were cards upon which were printed the names of the objects to be decided upon from various points of view. Of these there were forty pairs, fifty groups of three each, and fifty groups of five each.[1] The following are some representative examples.

---

[1] All the pairs, twenty-five groups of three and twenty-five groups of five were selected from material prepared by G. W. Peckham, who had begun work upon this problem in the Harvard Laboratory, but found it necessary to discontinue the investigation.

Pairs: violin music, piano music; Hamlet, Macbeth; orange marmalade, baked apple; Hume, Kant; gunpowder, printing; camel, elephant. Threes: Poe, Kipling, Hawthorne; wealth, strength, power; literature, science, philosophy; baseball, football, tennis; Dante, Goethe, Shakespeare; X-rays, photography, telegraphy; fountain pen, penknife, watch. Fives: music, sculpture, painting, drama, poetry; peaches, pears, apples, grapes, cherries; chrysanthemum, lily, dahlia, carnation, rose; Rembrandt, Holbein, Titian, Rubens, Ingres; temperance, wisdom, justice, courage, truth; bicycle, safety razor, automobile, telephone, balloon. The material used for subjective decision in the second part of the investigation, and that used for accuracy of decision, and in the several tests, will be described under the sections dealing with these various experiments.

RELATION OF TIME AND CONSTANCY

The first experiments performed were to determine the relation between time and constancy in relatively subjective decisions. These experiments can be divided into three series. In the first series, consisting of forty experiments, the pairs of cards, described above, were exposed by tilting back a shutter behind which they had been placed. The subject was directed to decide as quickly as was compatible with sincerity, and to raise the right or left hand according as the decision was for the right or left card. The basis of decision, which varied with the nature of the concepts, was in each case given by the experimenter; thus: decide according to your preference, their greatness, their importance, or their usefulness. The time was taken with a stop watch, which was started at the same instant that the crank of the shutter was turned back, and stopped at the signal of the subject. The time and the decision were recorded. After five weeks the experiment was repeated in the same way. The decision and time were again recorded and compared with those of the first presentation.

The results of this series of experiments are shown in Table I. The number of changed decisions, the average time for the first presentation, the average time for the second presentation, the combined average, and the number of first decisions remembered at the second presentation, are given for each of the nine subjects. The number of decisions changed varies from 1 for subject F to 13 for subject G; and the decision time from 1.46″ for C to 3.30″ for H. There is, however, no evidence of a correlation between time and constancy. The figures in the last column give some idea of the part played by memory in this experiment.

TABLE I

Results with Pairs

Subject	*No. C.*	*A. T.*^{1}	*A. T.*^{2}	*A. T.*^{3}	*No. R.*
F	1	2.96	2.34	2.65	31
A	4(—1)	2.74	2.56	2.65	9
B	5(—2)	2.28	2.91	2.60	7
E	5	2.54	2.23	2.39	7
C	6(—1)	1.46	1.57	1.52	3
H	7	3.30	3.12	3.21	18
D	8(—1)	2.49	1.99	2.24	12
I	10(—1)	2.00	1.43	1.71	20
G	13(—3)	3.04	2.75	2.89	14

No. C. = Number of changed decisions. Those in brackets doubtful.
A. T.^{1} = Average for first presentation.
A. T.^{2} = Average for second presentation.
A. T.^{3} = Combined average.
No. R. = Number of decisions remembered from first to second presentation.

In order to eliminate the memory factor as far as possible, a more complex series of experiments was devised. This second series consisted of the fifty groups of three cards each. These were arranged by the subject *in order* of preference, greatness, importance, or usefulness. The method was otherwise the same as in Series I. The motor activity of arranging the cards was rendered approximately constant in the following manner: On tilting back the shutter the cards were exposed in an upright position between two slats. The subject removed each card and laid it down whether the presented arrangement was maintained or changed. This series was also repeated in four to five weeks, except in the case of subjects K, J and L; here the time between first and second presentation was seven to eight weeks The same directions for arranging the cards were given the subject on each occasion; but a change of the basis of decision sometimes occurred under the category specially mentioned,—e.g., *importance* might mean importance to self, to the world, to science, etc. The possibility of such changes was as far as possible avoided; but all differences due to these changes or to lack of knowledge of the material were eliminated. The results of this series are shown in Table II. The number of changed decisions, and the average, and the median for the first and for the second presentations are given. Also, in columns marked "Above M^2" and "Below M^2", the changes in decisions whose times are above or below the median in the second presentation are given. There are always more changes in decisions above the median. This may be explained by supposing that the uncertain decisions take the longest time, and are also the more liable to change.

TABLE II

Results with Groups of Three

Subject	*No. C.*	*A.*^{1}	*M.*^{1}	*A.*^{2}	*M.*^{2}	*Above M*^{2}	*Below M*^{2}
A	11	8.7	8.2	9.5	9.3	9	2
K	16(—6)	6.3	5.6	10.1	7.3	12	4
J	17(—5)	7.1	6.2	5.7	5.0	15	2
D	16	5.8	5.5	5.5	4.8	9	7
F	17	7.8	6.8	6.8	5.6	12	5
E	18(—1)	7.3	7.1	6.5	6.3	10	8
B	20(—3)	9.6	9.0	8.3	7.6	13	7
C	21(—4)	9.5	8.4	6.5	5.4	13	8
L	25(—7)	16.4	13.5	12.3	11.1	16	9
H	28(—5)	11.0	10.2	8.1	7.9	19	9
G	27	8.9	8.6	8.3	7.8	17	10
I	32(—2)	4.3	4.2	4.9	4.4	18	14
				(Total)........		163	85
						66%	34%

No. C. = Number of changed decisions.
A.^{1} = Average for first presentation.
A.^{2} = Average for second presentation.
M.^{1} = Median for first presentation.
M.^{2} = Median for second presentation.
Above M^2 and Below M^2 = Number of changed decisions above and below the median in the second presentation.
In Brackets, doubtful decisions to be subtracted.

Table III-A shows how the subjects might be classified according to type. The types, however, grade into each other and some subjects are difficult to place. The memory factor which vitiated the results of the first series (see table I) was almost negligible here, and entirely so in Series III, when the fifty groups of five cards each were presented and arranged as in Series II. These were also repeated in about five weeks and the results are summarized in Table IV in the same manner as in Table II. In Table III-B the subjects are classified according to type as a result of this series. For the most part they hold the same place as in III-A; but two subjects, E and F, are relegated from the constant to the inconstant class, probably confused by the greater complexity of the conditions. In the case of E, who was leaving the University, the second presentation for twenty-five of the fifty groups was given two weeks after the first instead of five. The number of changes would in all probability have been greater, had the time for him been the same as for the others. C, however, who is medium in time and constancy in III-A, takes comparatively very much longer to decide with groups of five; thus becoming slow in time and medium in constancy in III-B.

Subjects J, K and L are omitted in Table IV, because they were given only twenty-five groups of this series. Table V and III-C give the results of all the subjects for these twenty-five. On comparing Tables III-A and III-C it will be noticed that here again under the more complex conditions, one subject, K, is removed from the quick to the slow; and another, F, is removed from the quick constant to the slow inconstant. These were the twenty-five groups that were repeated with subject E in two weeks instead of five, and this probably accounts for his position in III-C as compared with III-B. It is worthy of notice that the slow subjects are on the whole more constant than the quick; but both slow and quick can be divided into constant and inconstant classes. The slow constant are, thus, more constant than the quick of the same class.

The classifications in Tables III-A, B and C were made more difficult by the fact that the medians of some subjects differ greatly from the averages. We have used the median as the basis of classification. This is quite satisfactory with subjects whose decision time is uniform, for here there is a close correspondence between median and average; but sometimes when the decision time is variable the correspondence is not so close. The median is always less than the average, sometimes only slightly less, but in certain cases (e.g., that of K) very much less. This suggests another classification of the subjects, on the basis of the mean variation of their decision times, into the "Uniform" and the "Variable." The most variable would then be subjects K, C and F; and the most uniform, subjects A, G, B and E. It will be noted that the subjects who were removed from one class to another under more complex conditions, as mentioned above, are also variable subjects; and their variability in time and change of class may be results of the same factor. For under any given class some decisions are more difficult than others; and their general inability to cope with difficult situations prolongs the decision time or produces inconstancy, even in circumstances when less difficult decisions are quick and constant. In other words, there is a type of subject who, in simple situations (i.e., easy decisions), is very quick and constant; but who, when the decisions are more difficult, becomes relatively slower and more inconstant. This fact is of considerable significance in vocational guidance, and situations of practical life, for such subjects must be eliminated if a selection of *unfailingly* quick constant workers is to be made.

The method of estimating the amount of the change when a group of three or five was arranged in a different way on the second presentation needs some explanation. Not the absolute changes of position, but the relative changes were considered. Thus, in a group of five, A, B, C, D, and E, the greatest possible number of changes is ten. This would occur if it were completely inverted, E, D, C, B, A; for although C is in the same absolute position, yet A has changed relatively to B, C, D and E, B to C, D, and E, C to D and E, and D to E. In one group of five, then, any degree of difference from one to ten is possible, while in a group of three, the difference cannot exceed three.

TABLE III-A

Threes

Type	*Subject*	**No. C.*	*M.*^{1}	*M.*^{2}
Slow constant	A	11	8.2	9.3
	B	17	9.0	7.6
Slow inconstant	H	23	10.2	7.9
	G	27	8.6	7.8
	L	18	13.5	11.1
Quick constant	C	17	8.4	5.4
	D	16	5.5	4.8
	E	17	7.1	6.3
	J	12	6.2	5.0
	K	10	5.6	7.3
	F	17	6.8	5.6
Quick inconstant	I	30	4.2	4.8

*The doubtful ones, bracketed in Table II, have been subtracted.

TABLE IV

Results with Groups of Five (50)

Subject	**No. C.*	*A.*^{1}	*M.*^{1}	*A.*^{2}	*M.*^{2}	*Above M.*^{2}	*Below M.*^{2}
A	36	19.7	19.0	20.1	19.6	23	13
B	54	20.3	19.6	20.7	21.0	33	21
C	66	23.6	19.7	15.9	13.4	40	26
D	66	13.7	12.1	14.0	12.5	48	18
E	73	14.9	14.6	13.0	12.8	45	28
F	77	18.5	16.1	15.7	13.4	55	22
G	77	19.5	18.8	17.0	16.8	40	37
H	79	24.2	22.2	18.7	18.3	58	21
I	108	8.6	8.6	8.3	7.5	45	63
					(Total)	387	249
						61%	39%

*All doubtful ones have been subtracted.

TABLE III-B

Fives—50 Groups

Type	*Subject*	*No. C.*	*M.*^{1}	*M.*^{2}
Slow constant	A	36	19.0	19.6
	B	54	19.6	21.0
	C	66	19.7	13.4
Slow inconstant	H	79	22.2	18.3
	G	77	18.8	16.8
Quick constant	D	66	12.1	12.5
Quick inconstant	F	77	16.1	13.4
	E	73	14.6	12.8
	I	108	8.6	7.5

TABLE V

Results with Groups of Five (25)

Subject	*No. C.*	*A.*^{1}	*M.*^{1}	*A.*^{2}	*M.*^{2}	*Above M.*^{2}	*Below M.*^{2}
A	23*(6)	21.4	21.3	21.0	20.0	16	7
E	24 (9)	15.0	13.6	13.2	13.0	16	8
B	27 (8)	20.9	20.2	20.0	19.0	16	11
J	29 (5)	14.6	13.2	12.6	11.0	17	12
D	29 (9)	13.4	12.4	15.2	12.4	19	10
C	30(10)	19.1	17.0	11.6	12.0	16	14
K	31 (5)	32.4	22.4	34.3	27.6	21	10
H	36 (9)	23.1	22.2	18.1	17.2	26	10
G	35(11)	17.9	15.6	15.4	15.0	17	18
I	36 (9)	10.0	9.0	10.0	10.0	25	11
F	45 (5)	21.6	22.0	22.2	24.1	31	14
L	48 (4)	22.1	20.0	16.5	15.6	29	19
				(Total)........		249	144
						63%	37%

*In brackets are *additional doubtful* differences.

TABLE III-C

Fives—25 Groups

Type	*Subject*	*No. C.*	*A.M.*
Slow constant	A	23	20.2
	B	27	20.0
	K	31	25.0
Slow inconstant	H	36	19.3
	G	35	15.6
	F	45	23.2
	L	48	17.8
Quick constant	E	24	13.2
	J	29	12.1
	D	29	12.4
	C	30	14.5
Quick inconstant	I	36	9.5

A. M. = Average of M.^{1} and M.^{2}
No. C. = Changes, all doubtful ones subtracted.

It will be noticed that the time for the second presentation is generally less than for the first. In some cases (e.g., that of subject C) it is much less. This is what we would expect, but there are two striking exceptions: viz., subject K, whose time is much longer in the second case, and subject A. This lengthening of the time may be due to a more or less conscious effort on the part of these subjects to be constant. The decrease in time in the case of C is very much more than one would expect; and for H and L considerably more than the average. Here we have a type of subject that, having once made a satisfactory decision, is able, when placed in a similar situation, to make a similar decision much quicker than the first. Such a subject learns from experience, and performs his customary decisions as it were automatically. So long as the situation remains the same he becomes more and more efficient. This, however, does not prove that he will be more efficient in *novel* situations. On the contrary, the latter sort of efficiency may very well be an original endowment, that no amount of training can develop. The time, therefore, for the first presentation is of greater significance in classifying the subjects than that for the second. Here again the existence of these two types of efficiency in decision has considerable bearing upon vocational guidance.

The mean average decision time was determined for pairs, for groups of three, and for groups of five. The columns marked A in Tables I, II, and V were added up; and the respective sums divided by the number of subjects in each case. The results are as follows:

Mean average time for pairs	2.54
Mean average time for threes	8.56
Mean average time for fives	19.30

The ratio of increase is greater from pairs to threes than from threes to fives (3.37 and 2.14 respectively). The subjects whose decision times vary most from the average are: I, whose time for groups of five is 8.6, and K, whose time for the same is 32.4. The former is a "Uniform" subject, the latter a "Variable" one; and there is, therefore, no correlation between variability of decision time for any particular subject and variability from the mean average of all the subjects. We are speaking of variability in time only, not in decision; and the above has, therefore, no bearing upon F. L. Wells' statement that "Those who vary the least from their own judgments also vary least from the judgments of others."[2]

---

[2] "On the Variability of Individual Judgments," Essays Phil. and Psych. in honor of William James, p. 529.

A noticeable feature in the second presentation of Series II and III was the numerous memory illusions. These occurred with every subject but with some more than others. The women were generally more subject to them than the men. In each case the subject was asked whether he rememberd having made the decision before, and if so whether he decided in the same way. In many cases the subject would vigorously affirm that he actually remembered arranging the cards in the same way before, when the previous arrangement was really quite different. There were also some fewer cases when the subject remembered that his decision was different, when it was really the same.

CONFIDENCE

Three of the subjects—J, K, and L—were asked to state the degree of their confidence in each decision. This was reported by the subjects as meaning either "The subjective feeling of satisfaction in the arrangement," or "The assurance of constancy." The same subject would alternate between these two attitudes from time to time. The subjective feeling is different in each case; but for our purposes, the correlation of constancy and confidence, we assume that they need not be distinguished; for the former always implies the latter, though the opposite is not always true. The degree of confidence was given as A, B, or C,—A being perfect confidence, C very little or none, and B medium. It is clear that the subjects might differ not only in the degree of their confidence but also in their way of describing it. The same degree of confidence might be described as A by one and as B or C by another. This, however, is a criticism that can be raised as well against the introspective method as a whole.

The results of this confidence test are shown in Tables VI-A and VI-B. The subjects are compared with one another; and the confidence of the decisions that were changed with that of the whole. The "numerical value" is obtained rather arbitrarily by valuing A at 2, B at 1, and C at 0; but perhaps it is not so arbitrary after all, for A is introspectively perfect confidence (1), B medium (½), and C none (0); and to avoid fractions 2, 1, and 0 are used instead. The average confidence is obtained by dividing the numerical value by the number of decisions.

The following points are noteworthy:

(1) The more confident subjects are also the more constant.

(2) The confidence is slightly less with the decisions that were changed. (See Tables VI-A and VI-B under "Average Confidence.")

(3) One subject (J) is regularly less confident on the second presentation; the others are equally or more confident.

(4) The decisions changed are divided fairly equally among A, B, and C; but the amount of the change is as a rule greater in C decisions than in B's or A's. There are, however, exceptions. (See tables VI-A and VI-B, last section.)

TABLE VI-A—(THREES)

Degree of Confidence in Total Decisions

Subject	*No. D.*	*P.*^{1}	*P.*^{2}	*N. V.*^{1}	*N. V.*^{2}
J	49	32A + 10B + 7C	26A + 15B + 8C	74	67
K	49	28A + 13B + 8C	29A + 16B + 4C	69	74
L	50	14A + 25B + 11C	14A + 29B + 7C	53	57

Degree of Confidence in Decisions Changed

Subject	*No. D.*	*P.*^{1}	*P.*^{2}	*N. V.*^{1}	*N. V.*^{2}
J	10	6A + 3B + 1C	2A + 6B + 2C	15	10
K	9	3A + 4B + 2C	4A + 4B + 1C	10	12
L	16	4A + 6B + 6C	4A + 8B + 4C	14	16

Average Confidence, if A is represented by 2

	Total Decisions		*Decisions Changed*	
	P.^{1}	*P.*^{2}	*P.*^{1}	*P.*^{2}
J	1.51	1.37	1.50	1.00
K	1.41	1.51	1.11	1.33
L	1.06	1.14	.87	1.00

Average Amount of Change for A, B, or C Confidence

	No. Groups Changed		*N. V. of Changes*		*Ave. for A, B, C*
J	10	6A	12	7A	1⅙ A
		3B		3B	1 B
		1C		2C	2 C
K	9	3A	10	3A	1 A
		4B		4B	1 B
		2C		3C	1½ C
L	16	4A	18	4A	1 A
		6B		6B	1 B
		6C		8C	1⅓ C

P.^{1} and P.^{2} = First and second presentations.
N. V.^{1} and N. V.^{2} = Numerical Value of confidence in first and second Decisions
No. D. = Number of Decisions.

TABLE VI-B (FIVES)

Degree of Confidence in Total Decisions

Subject	*No. D.*	*P.*^{1}	*P.*^{2}	*N. V.*^{1}	*N. V.*^{2}
J	25	14A + 6B + 5C	9A + 13B + 3C	34	31
K	25	10A + 12B + 3C	11A + 9B + 5C	32	31
L	25	3A + 14B + 8C	2A + 19B + 4C	20	23

Degree of Confidence in Decisions Changed

Subject	*No. D.*	*P.*^{1}	*P.*^{2}	*N. V.*^{1}	*N. V.*^{2}
J	14	5A + 4B + 5C	2A + 10B + 2C	14	14
K	17	5A + 10B + 2C	7A + 5B + 5C	20	19
L	21	2A + 11B + 8C	1A + 16B + 4C	15	18

Average Confidence, if A is represented by 2

	Total Decisions		*Decisions Changed*	
	P.^{1}	*P.*^{2}	*P.*^{1}	P.^{2}
J	1.36	1.24	1.00	1.00
K	1.28	1.24	1.18	1.12
L	.80	.92	.71	.86

Average Amount of Change for A, B, or C Confidence

	No. Groups Changed		*N. V. of Changes*		*Ave. for A, B, C*
		5A		6A	1⅕ A
J	14	4B	29	11B	2¾ B
		5C		12C	2⅖ C
		5A		6A	1⅕ A
K	17	10B	31	23B	2 3/10B
		2C		2C	1 C
		2A		8A	4 A
L	21	11B	48	20B	1 9/11B
		8C		20C	2½ C

The degree of difficulty of the decisions was also obtained from the same three subjects, but only for the first presentation. The results are given in Tables VII-A and VII-B. D means difficult, M medium, and E easy. An arbitrary "numerical value" is obtained by valuing the difficulty of D at 2, of M at 1, and of E at O; and the average difficulty by dividing the numerical value by the number of decisions.

It will be noticed that:

(1) The subjects differ considerably in the amount of difficulty, and the least confident subject has the greatest difficulty in the threes, but the least in the fives.

(2) The easy as well as the difficult decisions are changed, but the average difficulty for those changed is slightly greater than for the total.

TABLE VII-A (THREES)

Subject	*No. D.*	*Degree of Dif.*	*N. V.*	*Ave. Dif.*
		Degree of Difficulty in Total Decisions		
J	49	15D + 12M + 22E	42	.86
K	49	10D + 16M + 23E	36	.73
L	50	22D + 7M + 21E	51	1.02
		Degree of Difficulty in Those Changed		
J	10	4D + 2M + 4E	10	1.00
K	9	3D + 1M + 5E	7	.77
L	16	7D + 3M + 6E	17	1.06

TABLE VII-B (FIVES)

		Degree of Difficulty in Total Decisions		
J	25	8D + 7M + 10E	23	.92
K	25	13D + 4M + 8E	30	1.20
L	25	6D + 7M + 12E	19	.76
		Degree of Difficulty in Those Changed		
J	14	7D + 5M + 2E	19	1.36
K	17	10D + 3M + 4E	23	1.35
L	21	5D + 6M + 10E	16	.76

ASSOCIATION

Two experiments were carried out to ascertain the association time of the subjects. There were nine subjects, six of whom were the same in each case and three different. In the first a list of fifty words were used. These were repeated to each subject who was directed to reply as quickly as possible with the first word that came to his mind. The time was taken with a stop watch visually; that is, the watch was started and position of the hand noted when the word was given, and when the reply was heard. This method was found difficult and inaccurate; and in the second experiment, which consisted of forty words, the watch was started when the word was given and stopped when the reply was heard. The results were very much the same except in the case of C, whose time in the second experiment is much shorter than in the first. Full results of both experiments are given in Table VIII.

TABLE VIII

	Association Time					*Decision Time*		*Constancy*		
						A.^{1}		*No. C.*		
Subject	*A.*^{1}	*M.*^{1}	*A.*^{2}	*M.*^{2}	*T. S. C.*	*(3's)*	*(5's)*	*('3s)*	*(5's)*	
B	1.880	1.8	1.465	1.4	41.4	9.6	20.3	17	*54	†27
H	1.436	1.4	.....	...	....	11.0	24.2	23	79	36
F	1.416	1.4	1.387	1.4	32.4	7.8	18.5	17	77	45
G	1.348	1.4	.....	...	....	8.9	19.5	27	77	35
I	1.108	1.0	1.332	1.3	26.0	4.3	8.6	30	108	36
J	.....	...	1.147	1.2	35.2	7.1	14.6	12	..	29
K	.....	...	1.130	1.1	23.2	6.3	32.4	10	..	31
L	.....	...	1.100	1.1	32.0	16.4	22.1	18	..	48
C	1.820	1.8	1.057	1.0	27.6	9.5	23.6	17	66	30
A	1.092	1.0	1.040	1.0	34.0	8.7	19.7	11	36	23
E	1.068	1.0	.....	...	....	7.3	14.9	17	73	24
D	1.056	1.0	.987	1.0	32.6	5.8	13.7	16	66	29

A.^{1} = Average association time in first experiment.
A.^{2} = Average association time in second experiment.
M.$_1$ = Median for first experiment.
M.^{2} = Median for second experiment.
T. S. C. = Time for sorting cards.
A.^{1} = Average decision time for first presentation.
No. C. = Number of changes.
* = For fifty groups of fives.
† = For twenty-five groups of fives.

The amount of the correlation between association time and decision and constancy time may be brought out by the "Method of Unlike Signs." This method is recommended by Whipple only for preliminary survey and not for "final determinations of important correlations, because the probable error is too large."[3] It seems, however, sufficient for our immediate purpose.

The central tendency (median) of the association times of the twelve subjects is obtained, using the times of column 4 except for the subjects not represented therein. In Table VIII-A the subjects are arranged in the same order as in Table VIII, and their deviations above (+) or below (—) the central tendency (1.15) recorded. A similar procedure is followed for the decision time with threes (column 6) and with fives (column 8). By comparing each subject's deviation from the central tendency in decision time for threes with his deviation from the central tendency in association time (see Table VIII-A) it will be found that there are 50% cases of unlike signs; but a similar comparison in the case of decision time for fives gives 66 ⅔% cases of unlike signs. Now if we refer to Whipple's Manual, Table 9, we see that this means a correlation coefficient of 0 and —.509. The number of cases investigated is probably too small to warrant any inference from these figures.

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[3] G. M. Whipple, Manual of Physical and Mental tests, p. 40.

There is some evidence of a considerable correlation between association time and constancy. If we compare the deviation from the central tendency in the number of changes with the deviation in association time, we get for threes 17% cases of unlike signs, for fives 33 ⅓%. By reference to the Table in Whipple's Manual, we see that this signifies correlations of +.860 and +.509. It is scarcely probable that mere chance could give us these figures; and, though investigations on a larger number of subjects may alter them considerably, it is very likely that here we have evidence of a real correlation. We are, therefore, if we use the results of this very inadequate investigation, able to say that if a subject's association time is longer than the average, he is very liable to be more inconstant than the average; and similarly, that if his association time is shorter than the average, he is probably more constant; but in either case we can say nothing regarding the length of his decision time.

TABLE VIII-A

Subject	*D.A.*	*D.D.^{3}*	*D.D.^{5}*	*D.C.^{3}*	*D.C.^{5}*
B	+ .25	+ 1.3	+ .7	0	— 3½
H	+ .25	+ 2.7	+ 4.6	+ 6	+ 5½
F	+ .25	— .5	— 1.1	0	+ 14½
G	+ .25	+ .6	— .1	+ 10	+ 4½
I	+ .15	— 4.0	— 11.0	+ 13	+ 5½
J	+ .05	— 1.2	— 5.0	— 5	— 1½
K	— .05	— 2.0	+ 12.8	— 7	+ ½
L	— .05	+ 8.1	+ 2.5	+ 1	+ 17½
C	— .15	+ 1.2	+ 4.0	0	— ½
A	— .15	+ .4	+ .1	— 6	— 7½
E	— .15	— 1.0	— 4.7	0	— 6½
D	— .15	— 2.5	— 5.9	— 1	— 1½
		50%	66 ⅔%	16 ⅔%	33 ⅓%

D. A. = Deviation from central tendency (1.15) in association time.
D. D.^{3} = " " " " (8. 3) " decision time (3's).
D. D.^{5} = " " " " (19.6) " " " (5's).
D. C.^{3} = " " " " (17) " constancy (with 3's).
D. C.^{5} = " " " " (30½) " " (" 5's).
Numbers at foot of columns are percentages of unlike signs, by comparing with column 1.

SORTING CARDS

An experiment to test the rapidity of sorting cards was also performed. At first ordinary playing cards were used; but, since some of the subjects were more familiar with these than others, another pack of cards equally unfamiliar to all was substituted. This pack contained fifty-two cards, thirteen of which were marked A, and an equal number B, C, and D. The subject was told to sort them into four heaps according to the letters; and the time was taken with a stop watch. The results are shown in Table VIII, under column T. S. C. If the method of unlike signs be employed, as with association time above, to determine the correlation between time for sorting cards and decision time, it will be found that there are just 50% cases of unlike signs, and, therefore, the coefficient of correlation is zero.

MEMORY

Two sets of experiments were performed to test the memory of the subjects. The first was for immediate memory, and the second for memory over longer periods of time. In the former, nonsense syllables, words and figures were used. There were two tests with nonsense syllables. The first was to get the subject's memory limit; that is, the greatest number of syllables that could be remembered and repeated in order after two repetitions. The metronome was set to beat at one per second, and a syllable was exposed through a slot in a cardboard at each beat. Each list was given twice and the subject read the syllables aloud. After a five-second pause, the subject was asked to repeat the syllables in order. Four syllables were given in the first list; and if these were remembered, five were given, etc. In Table IX, column L.[1] shows the various limits. There is very little variation here, the limit being always four or five. Another test was therefore given. A list of nine syllables was exposed twice in precisely the same manner; and after five seconds the subject was asked to repeat all he could remember. The variations in this test were greater (from three to seven), as is shown by column "9," Table IX.

Two similar tests were made with words; one to ascertain the limit, the other to get the number remembered out of a list of fourteen. The method was the same as with the nonsense syllables, and the results are shown in columns L.^{2} and "14," Table IX. The limit varies from five to nine, and the number remembered out of fourteen, from eight to fourteen.

Memory for figures was tested by exposing numbers of six digits and upwards. The subject read the digits aloud at the beat of the metronome (one per second). The number was read through in this way twice, and then after a five-second interval it was repeated from memory. If the subject was successful, a number of one more digit was given, etc., until the limit was reached. The results varied from eight to eleven, as shown in column F, Table IX. The "Total" memory ability of each subject is obtained by adding the results of these five tests; and the subjects are arranged in order according to this sum. The decision type of each subject is indicated by the letters opposite the general memory index. More accurate information regarding time and constancy can be obtained in Tables II, IV or VIII.

If we again apply the "Method of unlike signs" to ascertain the amount of the correlations between the general memory index and decision time, and also between the former and constancy, we get in each instance 50% cases of unlike signs, and, therefore, a correlation of zero. Further, if the memory for nonsense sylla-

TABLE IX

			Immediate Memory				
	Nonsense Syl.		*Words*		*Figures*		*Decision*
Subject	*L.*^{1}	*9.*	*L.*^{2}	*14.*	*F.*	*Total*	*Type*
F	4	3	7	8	8	30	S — I *
J	4	3	6	9	8	30	Q — C
G	5	4	6	9	8	32	S — I
A	4	5	5	10	10	34	S — C
B	5	3	6	14	9	37	S — C
I	4	7	5	13	10	39	Q — I
C	5	4	8	11	11	39	†Sor Q — C
K	4	4	8	14	10	40	†Sor Q — C
D	5	6	9	11	10	41	Q — C
L	5	7	9	14	9	44	S — I

S = slow.
Q = quick.
I = inconstant.
C = constant.
* = Q — C with threes.
† = Slow with fives.

bles, for words, and for figures be taken separately, we get in all *three cases* a coefficient of zero between memory and *decision time;* but a coefficient of .587 between memory for words and *constancy* in decision; and of .809 between memory span for figures after five seconds and *constancy.* That is, if we compare each subject's deviation from the central tendency for constancy with his deviation from the central tendency for memory for figures, we find that there are 80% cases of unlike signs. By referring to Whipple's Manual, Table 9, we see that this means a coefficient of correlation of —.809; but since + in the case of memory for figures signifies excellence, and in the case of number of differences, inconstancy, the relation between good memory and constancy is a direct (+) relation and not an inverse one. That is, the constant subjects have better memories for figures; and the same is true of memory for words.

Memory for a period of one week was also tested. Two lists of nonsense syllables of ten syllables each and one list of twelve words were given. The Lipmann-Marx memory apparatus[4] was used, and the number of repetitions necessary to learn each list recorded. The same list was given one week later and the saving in repetitions noted. The learning was called complete when the subject could anticipate one word or syllable ahead throughout the list. Learning ability or immediate memory is shown by the number of repetitions necessary to complete the learning, and memory for one week is represented by the percentage of saving. The results are scarcely reliable, especially for the words, as only one list was given; but they are presented in Table X and can be taken for what they are worth. The average of the two lists of nonsense syllables is taken and the percentage gained is calculated. The subjects are arranged (X-A) in order of the saving for the nonsense syllables; but no definite relation is shown to time and constancy which are indicated in the last column. If, however, they are arranged according to the saving for words (X-B), the slow appear to have poorer memories than the quick. There was some appearance of a similar relation in the case of immediate memory; but an application of the "Method

[4] See Bericht Über den VI Kongress für experimentelle Psychologie, p. 308.

of Unlike Signs" failed to reveal it. The same method now brings out a correlation coefficient of .92 between quickness in decision and memory for a period of one week; but it shows no correlation of memory for words with constancy; and in the case of nonsense syllables, no correlation of memory for one week with either constancy or time of decision. There are large individual differences in the number of repetitions necessary to learn the lists. These might be considered with Table IX as another test of immediate memory. No definite correlation is, however, brought out between the learning of either nonsense syllables or words, and decision time and constancy.

TABLE X-A

Memory for One Week

	Nonsense Syl.			*Words*			
Subject	*R.*^{1}	*R.*^{2}	*% Gained*	*R.*^{1}	*R.*^{2}	*% Gained*	*Decision Type*
J	12	10	17	11	6	45	Q — C
L	6½	5	23	10	6	40	S — I
A	10½	7	33	7	7	0	S — C
I	8½	5	41	4	2	50	Q — I
B	10½	6	43	8	5	38½	S — C
K	14½	7	52	7	5	28	S — C (or Q — C)
C	12	5½	54	8	4	50	Q — C (or S — C)
F	25	7½	70	6	3	50	S — I (or Q — C)

TABLE X-B

	% Gained on Words	*Decision Type*
A	0	S — C
K	28	S — C (or Q — C)
B	38½	S — C
L	40	S — I
J	45	Q — C
C	50	Q — C (or S — C)
I	50	Q — I
F	50	Q — C (or S — I)

R.^{1} = Repetitions required for the first learning.
R.^{2} = Repetitions required for the second learning.

ACCURACY OF DECISION

The foregoing experiments on decision dealt with the constancy of the subjects in subjective judgments, but no question of accuracy was involved. It, therefore, seemed advisable to perform some experiment to test the accuracy of decision. It proved however, very difficult to devise an experiment that would test accuracy of decision, and that would not at the same time involve a test of sensory acuity. The following method, which is not free from various objections, was adopted. Black paper cards, in which different numbers of small holes were made, were exposed two at a time by means of a drop apparatus, and the subject was asked to point as quickly as possible to the one having the greater number of holes, or to raise both hands if he considered them the same. Two cards (A and B) with twenty-five holes in each were used as standards; and the other cards, containing from twenty to thirty holes each, were exposed successively with one or the other of these. The standard was placed now on the right side, now on the left; and the apparatus was so placed that when the slide dropped and the cards were exposed, the light shone through the holes from a window in the rear. The time was taken with a stop watch, and fifty decisions in all were obtained. The results are shown in Table XI under A.

It might have been expected that the subjects would fall into four classes; viz., slow accurate, slow inaccurate, quick accurate, and quick inaccurate; and that these would correspond to the four types of subjective decision. As is shown by Table XI, the latter is not altogether true. The types in subjective and objective decision are indicated by the letters in juxtaposed columns. The quickness of the objective decision is based upon the median rather than the average, as the latter was sometimes greatly increased by a few unduly prolonged decisions.

The difference in the number of errors are, however, scarcely sufficient to justify a classification into accurate and inaccurate. Fifty more decisions were, therefore, obtained. In this case the cards used as standards contained fifty holes each, and the other cards from forty-four to fifty-five. An attempt was made to have the holes cover nearly equal group-areas on the cards. The method of procedure was the same as before. The results are shown in Table XI under B. There is even less correspondence here between the subjective and objective types; e.g., subject I, who is quick and inconstant in subjective decisions, is slow and accurate in objective, and A, who is slow and constant in the former, is quick and inaccurate in the latter, etc.

TABLE XI

Subject	*E.*	*A.*	*M.*	*Type in Obj. Dec.*	*Type in Subj. Dec.*
			A—Fifty Easy Decisions		
D	13	1.50	1.4	Q—A	Q—C
C	13	1.56	1.4	Q—A	Q or S—C
A	13	1.98	1.8	S—A	S—C
I	14	2.07	1.6	Q—I or A	Q—I
F	15	1.78	1.6	Q—I	S—I (or Q—C)
B	16	2.76	2.2	S—I	S—C
			B—Fifty Difficult Decisions		
I	16	3.8	3.6	S—A	Q—I
F	18	2.5	2.5	Q—A	S—I (or Q—C)
C	19	3.4	2.7	Q—A	Q—C
A	23	2.3	2.0	Q—I	S—C
B	24	5.7	4.3	S—I	S—C
			C—Twenty-five Difficult Decisions		
K	7	7.7	4.3	S—A	S—C (or Q—C)
J	9	4.0	3.6	S—A or I	Q—C
L	10	3.8	3.0	S—A or I	S—I

D—Twenty-five Dif. Decisions—All Subjects

Subject	*E.*	*A.*	*M.*	*Type in Obj. Dec.*	*Type in Subj. Dec.*
K	7	7.7	4.3	S—A	Q—C (or S—C)
I	8	3.8	3.6	S—A	Q—I
J	9	4.0	3.6	S—A	Q—C
C	10	3.4	2.7	Q—A or I	Q—C (or S—C)
L	10	3.8	3.0	S—A or I	S—I
F	11	2.5	2.5	Q—I	S—I (or Q—C)
B	11	5.7	4.3	S—I	S—C
A	14	2.3	2.0	Q—I	S—C

E. = errors.
A. = average time.
M. = median.
Q = quick.
C = constant.
I = inconstant, or inaccurate.
S = slow.
A = accurate.

Twenty-five of the difficult decisions were given to subjects K, L, and J, and the results are shown in Table XI-C. The times for all the subjects and the number of errors for the same twenty-five cards as were presented to these three are given in Table XI-D. An interesting result comes to light here; but it is also noticeable, though somewhat less evident, in the other Tables. It appears that *all those who were quick in subjective decisions are accurate in objective decisions, and the slow in subjective decisions are inaccurate in objective.*

This is a rather interesting correlation; but on second thought it is not so extraordinary as it at first seems. The individual who can narrow down his field of consciousness upon one problem or situation, and who can keep his attention from associated ideas, will be quick in what we have been calling subjective decisions, for these are very rich in attractive associations. It is not surprising that such a mind will also be *accurate* in objective decisions, especially the kind we have required,—for here suggestibility and liability to illusions are prominent factors in producing error. It is very probable that suggestibility is the basis of this correlation, and that the suggestible person will be inaccurate in objective decisions as well as slow in subjective decisions of the kind we have been using here. In the second part of this paper it is shown that subjects quick in aesthetic decisions (Q and K) may also be very suggestible.

Those who were quick in subjective decision are not, as might be expected, always quick in objective. Indeed, the opposite seems true; for, taking the objective decisions by themselves, the accurate subjects are as a rule slow.

Their time, however, varies considerably, and this is the important result from the practical point of view; for it shows that in the same situation, there are some subjects who can decide both more quickly and more accurately than others. We have all along been speaking of Decision Types; but it must be clear to anyone who examines the figures that there are really no separate and distinct types into which each individual must fall; but that the types, so-called, grade into each other. It is often difficult to classify a subject according to type, because he is intermediate; and if the experiment were performed upon a larger number of subjects all the apparent gaps would no doubt be filled up. When we speak of types we are doing so merely for convenience sake, and not with the intention of adopting a "Multimodel Theory."

In all probability, this test was complicated by various spatial illusions, and illusions in the estimation of number. C. T. Burnet, in his study of the "Estimation of Number,"[5] has pointed out the tendency of some subjects to over-estimate compact groups, and of others to over-estimate scattered groups. In the second series of decisions in our experiment, the group-areas are all the same. Hence the holes would be closer together in some than in others; and this was also true of the first series, though no attempt was made to have the group-areas the same.

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[5] Harvard Psych. Studies, Vol. 2, 1906, pp. 349-404.

Another illusion that tended to produce a constant error until corrected was as follows: When the holes were farther apart, it produced the illusion that the group-area was larger. The apparently larger group was then pointed to as having more holes, when in reality it was the same size and had less holes. This was pointed out by one of the subjects after it had been the cause of several errors in his own case. Afterwards it was noticed with other subjects.

If all the subjects knew of all the possible illusions, then the estimation of the amount of the illusion might still be regarded as an objective decision. But in this particular case some subjects knew more of the illusions involved than others. We, therefore, conclude that the liability of the subject to illusion was involved, perhaps as the most important factor. If this is true, then the above result would be stated thus: Those who are quick in subjective decisions are less liable to illusion; that is, less suggestible, as we have noted above.

CONFIDENCE

The confidence of the subject, whether A, B, or C, was ascertained for each decision, as in the subjective decisions reported above. Confidence in this case means "Degree of certainty that the judgment is correct." The "numerical value" and "average confidence" were ascertained as before. The results of this confidence test are summarized in Tables XII-A and XII-B. It will be noticed that the confidence of the subjects varies greatly; and that *the confident subject is, on the whole, not more apt to be correct than the inconfident one.* In fact, subjects F and I, who have the least average confidence, have the fewest errors in the fifty difficult decisions. On the other hand *the confident judgment of any given subject is more apt to be correct than that inconfident one,*—as is shown by a comparison of columns A. C. and A. C. E. There are great individual differences in this respect, some subjects having more confidence in erroneous judgments as compared with the total, than others: e.g., differences between A. C. and A. C. E. for C and K are small, while for J it is very large. The results here are slightly different from what we obtained for subjective decisions; but they possess greater reliability, because obtained from a greater number of subjects. Of course, both may be correct; that is, confidence may be correlated with consistency in subjective decisions; and at the same time show no correlation with accuracy in objective ones.

TABLE XII-A

First Series of Decisions—Easy

Subject	*N. E.*	*T. C.*	*A. C.*	*A. C. E.*
C	13	85	1.70	1.38
A	13	59	1.18	.92
D	13	54	1.08	.23
B	16	39	.78	.50
I	14	24	.48	.29
F	15	22	.44	0.

XII-B

Second Series of Decisions—Difficult

50 Dec.	C	19	73	1.46	1.16
	A	23	35	.70	.39
	B	24	17	.34	.17
	I	16	13	.26	.13
	F	18	5	.10	0.
25 Dec.	J	9	26	1.04	.11
	K	7	19	.76	.71
	L	10	15	.60	.30

N. E. = Number of errors.
T. C. = Total confidence—"numerical value."
A. C. = Average confidence.
A. C. E. = Average confidence for errors.

A comparison of the figures shows that no correlation between time in objective decisions, and association time or time for sorting cards is brought to light. Neither does there seem to be any correlation between memory ability and time; but a good memory is generally associated with accuracy. This is shown in Table XIII. It is also what we would expect; for we have seen that quickness in subjective decision is correlated with accuracy in objective decision and also with good memory.

TABLE XIII

Subject	*General Memory Index*	*Obj. Dec. Type*
F	30	Q — I
J	30	S — A
A	34	Q — I
B	37	S — I
I	39	S — A
C	39	Q — A (or I)
K	40	S — A
L	44	S — A (or I)

CHAPTER II

RELATION OF TIME CONSTANCY AND SUGGESTIBILITY IN AESTHETIC DECISIONS

The investigation was continued partly to verify the results already obtained, but chiefly to study the effect of suggestion upon decision time and constancy, and the correlation of suggestibility with these factors. Ten subjects participated in the investigation; and three of these, A, C, and L, had also taken part in the last. The same apparatus was used and the same method followed as in the previous experiment, except that this time only groups of five were used and instead of arranging them in order of preference the subject selected only the best and worst of the group.

In the previous investigation the names of the things to be decided upon were printed upon cards and exposed to the subject; this time the actual objects were used. These were: fifteen groups of colors, fifteen of color combinations, twenty of geometrical figures, twenty of pictures (reproductions of famous paintings), twenty of picture postal cards, and only the last ten of words as in the first experiment. In all one hundred decisions were obtained and these were, all except three or four in the last ten, aesthetic decisions. The subject was asked to place the best of the group on his left, the worst on his right. The five members of each group were arranged for each subject in the same way. They were marked A, B, C, D, E, and presented in this order from the subject's right to his left. The decisions for best and worst were recorded, and we were thus able to calculate the influence of position upon the subject's decision. In doing this we were obliged to omit the results for the first fifteen groups of colors and for five groups of geometrical figures, because for them the order of presentation was not recorded. The results for the remaining eighty decisions are given in Table XIV. Under the column marked "Pleasant" are given the number of times A, B, C, D, and E cards were chosen as best by each subject; under the column marked "Unpleasant," the number of times each card was chosen as worst by each subject; and under the column marked "Total," the number of times each card was chosen either as best or worst by each subject. The totals for all the subjects in these three columns are given in the horizontal "Total" line at the foot. By referring to the total for the "Totals" column, it will be seen that B (that is, one card from the subject's right) had the preference for position, being chosen 351 times; while A (the extreme right card) was chosen least often, 308 times; but there is much less difference here than one would expect. The C, D, and E cards were chosen 314, 313, and 314 times respectively.

If we now turn to the totals for the "Pleasant" and "Unpleasant" columns, we find evidence of a pronounced tendency to choose the cards towards the E end of the line as pleasant, and towards the A end as unpleasant; e.g., E was chosen 183 times as pleasant, 131 as unpleasant; while A, on the other hand, was chosen 162 times as unpleasant, 146 as pleasant; and B 192 times as unpleasant and 159 times as pleasant. It will be remembered that E was always on the subject's left and A on his right, and that the instructions were: "Place the best card on your left and the worst on your right." We have here, therefore, evidence of an *inertia* or tendency to leave things as they are. This inertia varies greatly with different subjects. In the table we have arranged them roughly according to its amount, which is determined by the sum of the differences between the number of times A and E were chosen as pleasant, and also as unpleasant.

TABLE XIV

	Pleasant					*Unpleasant*					*Totals*				
Subject	*A*	*B*	*C*	*D*	*E*	*A*	*B*	*C*	*D*	*E*	*A*	*B*	*C*	*D*	*E*
O	14	14	18	11	23	16	17	17	19	11	30	31	35	30	34
R	12	18	11	16	23	16	19	15	17	13	28	37	26	33	36
C	11	13	22	18	16	18	16	13	20	13	29	29	35	38	29
N	14	15	16	18	17	18	20	11	17	14	32	35	27	35	31
A	14	18	15	20	13	20	17	14	17	12	34	34	29	37	25
Q	14	15	18	14	19	18	22	13	10	17	32	37	31	24	36
P	15	16	20	8	21	15	16	18	16	15	30	32	38	24	36
S	19	18	11	13	19	14	22	16	17	11	33	40	27	30	30
M	18	14	15	14	19	16	21	10	17	16	34	35	25	31	35
L	15	18	18	16	13	11	22	23	15	9	26	40	41	31	22
Total	146	159	164	148	183	162	192	150	165	131	308	351	314	313	314

It would seem, then, that the influence of position alone, judging from the totals for all subjects, is almost a negligible factor in this experiment. To be sure, B was chosen thirty-seven times oftener than any other card; but this predominance is almost entirely accounted for by its greater frequency in the "Unpleasant" group, where it appears twenty-seven times oftener than any other card. The influence of position *plus* instruction to place the best on the left and the worst on the right is, however, very marked, and, as we have pointed out, more noticeable with some subjects than others. This influence of position plus instruction may perhaps be explained in terms of suggestibility. If so, the most suggestible subjects would be most susceptible to it. This, however, does not seem to be the case; for a later arrangement of the subjects according to suggestibility does not follow the one given here. It may be, of course, that the later arrangement is at fault; but, on the whole, it would seem better to consider this result due to a tendency more pronounced in some subjects than others to leave things as they are when a decision would be difficult.

The influence of position alone, which may truly be called suggestion, also varies greatly with different subjects. This difference is brought out by a comparison of the figures for the various subjects in the "Totals" columns. In the case of each subject, the position least often chosen is subtracted from that most often chosen. The difference is taken as a numerical value of the influence of position, and in Table XV the subjects are arranged according to its amount. There seems to be an inverse correlation between suggestibility for position and the inertia discussed above; for in Table XIV, the subjects were arranged according to this inertia from the greatest to the least; here they are arranged from the least suggestibility for position to the greatest, and their relative positions are only slightly changed. This adds weight to the view that the influence of position plus instruction is not due to suggestibility, but to what we have termed inertia. The arrangement according to suggestibility for position does not, however, correspond to that later obtained for suggestibility. This lack of correlation may arise because either one or both of the experiments are inadequate as tests for suggestibility. It is, of course, in perfect harmony with the view that there is no *general suggestibility,* but only varying *degrees of suggestibility* for different things.

In Table XV, column D, are given the differences referred to above; in column M, the position of the card most often chosen; and in column L, the position of the card least often chosen. It will be noted that the subjects differ considerably as to the position which attracts them most and least. Five of the subjects find B the most attractive position; three, C; and two, D. It is remarkable that neither A nor E, the two end cards, find a place. They occupy a mediocre position; for E occurs only twice and A once in the "least frequent" column. In the latter column, C occurs five times, each time being the least attractive position to a subject who found B the most attractive. B occurs in the L column not at all; and this position, that is, one from the right, has, therefore, the greatest attraction for the attention. If the instructions had been merely "select the best and worst" instead of "place the best and worst at your left and right," the attractiveness of the various positions might have been different; for the words "left" and "right" may have produced some effect upon the attention of the subjects.

TABLE XV

Subject	*D.*	*M.*	*L.*
O	5	C	A, D
N	8	B, D	C
C	9	D	=
M	10	B, E	C
R	11	B	C
A	12	D	E
Q	13	B	D
S	13	B	C
P	14	C	D
L	19	C	E

D = Difference between the number of Decisions for the card most often chosen, and the number for the card least often chosen.
M = Position most often chosen.
L = Position least often chosen.

Each of these hundred groups was presented to each subject for a second decision, after a period of six to eight weeks. Fifty of them were presented in precisely the same manner as at first. The other fifty were presented each time with a suggestion to the subject to choose certain cards. The number of times that each card was chosen as best or worst in the first presentation was ascertained for each of the hundred groups. Sometimes the same card was chosen as best or worst by almost all of the ten subjects; e.g., colors, group 4, pleasant: 7A, 0B, 2C, 0D, 1E; unpleasant: 2A, 0B, 1C, 0D, 7E. This means that seven subjects chose A as pleasant; none, B; two, C; none, D; one, E; and similarly for the unpleasant. In other groups, a card that was chosen best by several subjects, was chosen worst by the rest; e.g., pictures, group 18, pleasant: 2A, 4B, 3C, 0D, 1E; unpleasant: 1A, 5B, 1C, 2D, 1E. In many groups, however, there was a much greater division of opinion; e.g., post cards, group 8, pleasant: 1A, 3B, 0C, 6D, 0E; unpleasant: 4A, 1B, 1C, 0D, 4E. Groups of the last kind were as far as possible, selected for "suggestion groups." These groups were presented at various intervals between "non-suggestion groups," sometimes alternately and sometimes two or three of one kind successively and then one of the other. With the non-suggestion groups the instruction was as in the first presentation: "Place the best on your left, the worst on your right"; but with the suggestion groups the following was always added: "The card most often chosen best is at your left, that most often chosen worst on your right." That is, it was explained to the subject that in certain groups the cards as presented to him would be arranged in a special order, so that the card chosen best by the majority of the ten subjects would be presented to him at the left, the worst on his right. This constituted a suggestion to the subject to leave the cards as they were, but he was left free to make a change if he so desired. This suggestion was adopted because it seemed to be one that could be used continually throughout the investigation, while its effect would remain practically undiminished.

The cards actually placed on the left and right in the suggestion groups were sometimes those chosen best and worst by the majority, sometimes not; e.g. ,in the illustration of a suggestion group above, if a subject had previously chosen B as the best and E as the worst, he would be given D as the best and A as the worst; that is, a card that was really chosen the majority of times as best, on the left; and a tie for majority choice, on the right. If, however, the subject, himself, belonged to the class constituting the majority (that is, if he had chosen D as the best and A as the worst), he would be given B and E or C. The main object was never to place on the left and right the cards previously chosen by the subject himself as best and worst; the latter cards were always placed in positions B and D respectively. Thus, whenever the subject accepted the suggestion, he would have to change his previous decision to do so. We were thus able to calculate the effect of the suggestion, by comparing the number of changes in each subject's decisions for the fifty "non-suggestion groups" with his number of changes for the fifty "suggestion groups."

In Table XVI, the results are given for the fifty non-suggestion groups. The subjects are arranged according to the average decision time for the first presentation of the whole hundred groups. The average time for the second presentation of the fifty non-suggestion groups, and the number of decision changes in these groups are also given. In columns "a" and "d" are the number of changes in decision upon agreeable and disagreeable cards respectively. The "total" number of changes in the former is about the same as in the latter; but the individual subjects vary considerably. Subject R, e.g., has seven more changes in the "d" column, while Q and S have each nine more changes in the "a" column. This difference probably depends upon the amount of feeling produced by the agreeable or disagreeable cards. If the former tended to attract the attention more than the latter, there would be more differences in the choice of the latter, and vice versa. On the whole, the disagreeable cards were found by the subjects more unpleasant than the agreeable were pleasant, and thus the greater total number of differences in the "a" column is accounted for; but, as has been noted, there are marked individual differences here.

Turning now to a comparison of the total number of changes, we find that they range from 21 to 43. We have, then, as before, a constant and an inconstant class; but it is even more difficult here than in the previous investigation to draw any dividing line between these two classes. They grade into each other through a series of very small differences. The same is true of rapidity of decision. We get in column A. T.^{1} a very great range of decision time—from 9.1″ to 32.8″; yet it is difficult to draw a line separating the quick from the slow. There is a fairly large middle gap between C (14.6″) and P (17.0″); but with a larger number of subjects all gaps would probably disappear; and the dividing line between the fast and the slow would be purely arbitrary.

TABLE XVI

Subject	*A. T.^{1} (100)*	*A. T.^{2}= (50)*	*No. C=*	*(a*	*d)*
R	9.1	10.4	43	18	25
Q	10.8	12.5	43	26	17
A	12.7	12.3	21	8	13
S	13.0	13.9	39	24	15
C	14.6	8.7	21	11	10
P	17.0	13.5	32	19	13
M	18.1	14.8	30	15	15
L	20.8	16.9	35	17	18
O	24.5	17.1	29	13	16
N	32.8	27.2	37	17	20
			Total........	168	162

A. T.^{1} (100) = Average time for the first Decision on 100 groups.
A. T.^{2}= (50) = Average time for the second Decision on 50 "Non-suggestion groups."
No. C= = Number of Decision changes in Non-suggestion groups.
a and d = Number of changes in Decision for agreeable and disagreeable cards respectively.

The mean average time for all the subjects is less for these aesthetic decisions than for groups of five in the last investigation —17.34 and 19.30 respectively. It will be remembered that in the last experiment the five cards were arranged in order, while here only the best and worst were selected. One would suppose that this difference in requirement would have effected a saving of more than two seconds; but probably the selection of the first and the last is the most difficult part of the arrangement. The actual saving may be really larger than the figures show (i.e., 2″); because for the only three subjects that participated in both investigations it is 5.2″; and the figures of Table XIX seem to indicate that the effect of practice upon decision time is very small if not altogether absent. The saving for subjects A and C is much greater than for L, whose time is only slightly quicker in the aesthetic decisions. Of course, the difference in time may be due to difference in material, as well as to difference in the decisions required. The time for arranging a group of three in the last experiment was 8.5″. This, therefore, is about the difference we would expect between arranging a group of five in order, and selecting the best and worst from a group of five. The actual difference is about 3″ less than this. It is, therefore, probable that the aesthetic decisions on actual objects would, other things being equal, require more time than the previous more or less logical decisions.

As in the previous experiment, the decision time for the second presentation is usually less than for the first; but here again there are two exceptions, R and Q. Subjects C and L both decrease their time about as much relatively as before, showing that this is a constant tendency with them, as no doubt to increase, decrease, or remain the same is with every subject. These individual differences are very difficult to interpret; but see the discussion of this subject above (pp. 8-9).

The results for the second presentation of the fifty "suggestion groups" are given in Table XVII. The subjects are arranged roughly on the basis of suggestibility. The purely suggestible are placed first; then those that likewise give evidence of contra-suggestibility, and finally the non-suggestible. The average times for the "suggestion groups" are in column A. T.$^{2+}$. The 2 means second presentation, and the plus sign means "with suggestion." just as in Table XVI the "equals" sign means "without suggestion." In column C+ (a d), we have first the total number of changes in decision for the fifty groups, and then the number of changes in the decisions upon "agreeable" and "disagreeable" cards. Contrary to what we obtained in Table XVI, we see that the total number of changes is greater for the "d" column than for the "a" (220 and 203), which signifies that the *sugges-*

tion was accepted oftener in the case of the unpleasant than the pleasant choice; but here again there are some individual differences.

There is *less* variation in *time* and *more* in the number of changes here than in the "non-suggestion groups"; but we shall enter into a more detailed comparison of the two groups in the discussion of Table XVIII. Here we wish to bring out chiefly the method of determining the relative suggestibility of the subjects. The best way of doing this would seem at first glance to be to compare the results (especially the number of changes) for the suggestion groups with those for the non-suggestion groups. This procedure, however, presupposes that the number of changes would have been the same for each series, if there had been no suggestion; and we are not at all certain that with a limited number of cases this is true. A form of procedure somewhat less open to such a criticism is to consider the results of the suggestion groups by themselves. An illustration will best explain how suggestibility can be calculated in this way. Let us suppose that E, D, C, B, A, is a group, presented to the subject in this order from left to right; then E and A are the suggestion cards for best and worst respectively, B and D the subject's previous choices for best and worst respectively, and C the only neutral card. If B and D are selected as before, the subject is constant; but he produces a change by any of the following decisions or combinations thereof: (1) by accepting the suggestion; (2) by choosing C as either best or worst; (3) by reversing the suggestion,—that is, taking A as best and E as worst; and (4) by reversing his own previous decision,—that is, taking D as best and B as worst. The last possibility is practically negligible, as it occurred only once or twice in the whole investigation. No. 3 was taken as an evidence of contra-suggestibility; for the possibility of such a decision, if there were no suggestion, would be very low, since E and A were really chosen by at least three subjects, sometimes by many more, as best and worst respectively, and therefore most probably really differ in this way. This kind of decision is signified by the sign—; No. 2, showing non-suggestibility, by the sign =, and No. 1, showing suggestibility, by the sign +. Changes in decisions for both best and worst are counted two; a change in either one of them, one. The results are summarized in column C+, Table XVII. Subject R, e.g., has a total of 51, changes, 38 due to acceptance of the suggestion, 9 to non-acceptance, and 4 to reversal. Since acceptance is due to the choice of either of two cards (E or A), and non-acceptance to the choice of only one (C), there would be, without any suggestion, twice as many acceptances as non-acceptances. Therefore, suggestibility is shown in proportion as the + number in column C+ is more than twice as great as the =. Subject R, e.g., shows considerable suggestibility, for his + number (38) is more than *four* times as large as his = (9).

TABLE XVII

Subject	*A. T.*^{2}+ *(50)*	*C+*	*(a*	*d)*	*C+*			
R	10.0	51	27	24	38+	9=	4—	
Q	13.3	63	29	34	44+	15=	4—	Sug.
P	14.1	38	19	19	30+	7=	1—	
O	18.2	44	18	26	36+	4=	4—	
L	19.1	52	27	25	35+	8=	9—	Sug. and
C	10.0	39	19	20	19+	10=	10—	Contra-sug.
M	16.7	46	19	27	24+	15=	7—	
N	26.5	27	14	13	14+	8=	5—	Contra-sug.
A	12.9	30	12	18	17+	10=	3—	Non-sug.
S	14.1	33	19	14	22+	11=		
	Total........		203	220				

A. T.^{2}+ (50) = Average Decision time for "suggestion groups."
C+ (a d) = Total Decision changes, changes in choice of agreeable and of disagreeable cards.
C+ = For explanations of this column see pages 34, 35.
Sug. = Suggestible.

Contra-suggestibility is shown to some degree by all the subjects except S and P. The former is also not at all positively suggestible, while the latter is to a considerable degree. Subject L is both negatively and positively suggestible, while C, M, and N are only negatively suggestible. It would seem, then, that negative and positive suggestibility need not be inversely correlated in the individual but that any degree of one can coexist with any degree of the other. The fact that the same subject may be both positively and negatively suggestible is not at variance with ordinary observation. We all know individuals who alternate between periods in which they are easily influenced and periods of inflexibility.

There are, as we have intimated, other ways of estimating the suggestibility of the subjects from the results of our experiment. These are perhaps, less reliable than the one explained above, but they afford us very important auxiliary evidence regarding the status of the individual subjects; and also, since they depend upon a comparison of the results of the "suggestion" and "non-suggestion" groups, lead to a consideration of the influences of suggestion upon decision. This comparison is facilitated by the use of Table XVIII. Here the subjects are arranged according to the average time for the first decisions upon the hundred groups. In columns two and three are presented the average times for decisions upon the "non-suggestion" and "suggestion" groups respectively. In only two cases (R and N) is the time for the suggestion groups the shorter, and then only very slightly so. The other subjects differ considerably; for some (such as L, M and C) the time is relatively much longer for the suggestion groups; for others (Q, P, S, A, and O) it is only slightly longer. The lengthening of this time shows a general disturbance due to the suggestion, and probably an attempt to overcome it. It may, therefore be taken as an evidence of some degree of suggestibility; and it is noteworthy that it is most pronounced in the case of the subjects that show the greatest contra-suggestibility. That is, the process of resisting increases the decision time. A shortening of the time cannot, however, be taken as an evidence of the absence of suggestibility. It may, on the contrary, be explained as the result of an immediate yielding to the suggestion, as it seems to be, e.g., in the case of subject R. The subject who shows the least effect of the suggestion, viz., S, shows also the least difference in time.

The variation from the central tendency in decision time is greatest for the first presentation of the hundred groups, the limits being 9.1″ and 32.8″. It is less for the second presentation of the "non-suggestion groups," where the limits are 8.7″ and 27.2″; and it is least of all for the second presentation of the "suggestion groups," where the limits are 10″ and 26.5″. Hence the general effect of suggestion upon time in decision is to lengthen it, and reduce the variability of one subject from another.

Another method of calculating the relative suggestibility of the subjects is to compare the number of changes in non-suggestion and suggestion groups, as mentioned above (p. 34). These are given in Table XVIII, columns C= and C+ respectively. In only two cases (N and S) is the number of changes less in the C+ than in the C= column. The other subjects vary greatly, showing increases that range from 6 for M and P to 20 for Q. If we estimate the relative suggestibility of the subjects from the amount of the increase in the number of changes, the classification will be about like that of Table XVII, except that subject C will be more and P less suggestible.

As might be expected, the suggestions on the whole increased the inconstancy. It increased it, however, much more for some subjects than for others. The limits in the C= column are 21 and 43, while in the C+ column they are 27 and 63. This increase of inconstancy bears no marked relation to the inconstancy as determined by the fifty non-suggestion groups. Subject C, e.g., who was constant in the latter, has a relatively larger increase than subject R, who was inconstant; and the two subjects who actually decreased the number of changes were relatively inconstant. On the other hand, large *increases* were made by the relatively inconstant subjects Q and L.

In the second C+ column, we have presented the number of +, = and — changes as in Table XVII; and in column C=(+)C+, the total number of changes (+, = and —) for the suggestion and non-suggestion groups combined. A change in any of the latter groups is produced by the selection of any one of *three* possible cards (excluding the possibility of the subject reversing his own previous decision). All changes in non-suggestion groups are, of course, = changes. Taking then any two decisions, one without and one with suggestion, we have four possibilities of = changes, three in the former, one in the latter; and two of + changes, both in the latter. That is to say, in the whole hundred decisions we should have, barring the influence of suggestion, twice as many = as + changes; and the degree of suggestibility will be proportioned to the amount that the + changes exceed half the = changes. If we arrange the subjects on the basis of this calculation, we shall, on the whole, maintain the classification of Table XVII; but the positive suggestibility of subjects C and M and the negative suggestibility of subject N are thus better brought to light.

TABLE XVIII

Sub-ject	(*100*) *A.T.*^{1}	(*50*) *A.T.*^{2}=	(*50*) *A.T.*^{2}+	*C*=	*C*+	*C*+			*C*=(+)*C*+			*Sug.*
R	9.1	10.4	10.0	43	51	38+	9=	4—	52=	38+	4—	3+
Q	10.8	12.5	13.3	43	63	44+	15=	4—	58=	44+	4—	3+
A	12.7	12.3	12.9	21	30	17+	10=	3—	31=	17+	3—	2+
S	13.0	13.9	14.1	39	33	22+	11=		50=	22+		=
C	14.6	8.7	10.0	21	39	19+	10=	10—	31=	19+	10—	2+—
P	17.0	13.5	14.1	32	38	30+	7=	1—	39=	30+	1—	3+
M	18.1	14.8	16.7	30	46	24+	15=	7—	45=	24+	7—	2+—
L	20.8	16.9	19.1	35	52	35+	8=	9—	43=	35+	9—	3+—
O	24.5	17.1	18.2	29	44	36+	4=	4—	33=	36+	4—	3+
N	32.8	27.2	26.5	37	27	14+	8=	5—	45=	14+	5—	—
	17.3	14.7	15.5	330	423	…….. Total						
	Mean of Averages											

A. T.^{1} = Average time for first 100 Decisions.
A. T.^{2}= = Average time for second Decisions on 50 "non-suggestion groups."
A. T.^{2}+ = Average time for second Decisions on 50 "suggestion groups."
C= = Decision changes in non-suggestion groups.
C+ = Decision changes in suggestion groups.
For second C+ column and C=(+)C+, see pp. 37-38.
Sug. = Index of suggestibility.

There are thus three ways of calculating suggestibility if we omit the comparison of time, which has a rather ambiguous significance. If the suggestibility shown in any one of these ways is called 1+, an arbitrary index of suggestibility for each subject is obtained. This is given in column "Sug.", Table XVIII. 3+ means that suggestibility is shown by all three methods; 2+, by two; =, by none; 2+— means suggestibility by two methods coupled with contra-suggestibility; —, contra-suggestibility only, etc. The subjects seem to fall into four groups or types on the basis of suggestibility: (1) the positively suggestible; (2) the positively and negatively suggestible; (3) the non-suggestible; (4) the negatively suggestible. The last has only one representative (N) in this group of subjects. No. 3 seems to have only one also (S); but since for A the margin for suggestibility is very low in both methods in which it occurs, he should probably be placed in this type instead of No. 1. It is, of course, understood that these types are not distinct, but grade into each other as the "decision types" considered above.

There appears to be no correlation between suggestibility and decision time or constancy. There are quick and slow, constant and inconstant in the two types of suggestibility that are fairly represented; and this would probably also be the case with the other two, if there were more subjects. There may, however, be some significance in the fact that the contra-suggestible appear, on the whole, in the lower half of the column, and two of the most suggestible at the top. If so, it would mean that extreme quickness in decision time usually goes with suggestibility, and extreme slowness with contra-suggestibility, while the non-suggestible would be medium or fair in decision time. We have not, however, sufficient data in support of this view.

So far we have been considering the results of all the decisions combined. Let us now turn to a comparison of the results for the different kinds of objects decided upon, viz., colors, geometrical figures, pictures, post cards, and words. There were thirty groups of colors, ten of words, and twenty each of the others. Each subject's average decision time for each kind of material decided upon and also his average for the whole hundred are given in Table XIX. The central tendency of all the subjects for each series is also given at the foot of the table. On the whole, the subjects show a decrease of time from colors to geometrical figures, an increase from these to pictures, a still further increase to post cards, and a final decrease to words. The groups were presented to the subjects in this order; and if they were all of approximately equal difficulty, one would expect a decrease of time from one series to the next due to practice. The increase can, therefore, be accounted for only by the greater difficulty of the decisions upon pictures and especially post cards. It is most probable also that practice would have very little effect upon decision times.

The above states the general tendency; but there are very marked individual differences in the way the subjects vary in time from one series to another. Subjects R, Q, and A vary from the central tendency in showing an increase of time from colors to geometrical figures; C and M, in showing a decrease from geometrical figures to pictures; S, in showing a decrease from pictures to post cards; and R and L, in showing an increase from post cards to words. That is to say, although on the whole geometrical figures are the easiest to decide upon, yet subjects R, Q, and A find colors, and M, words, slightly the easiest, and C, the pictures much the easiest. These variations are significant, for they seem to show that decision times vary relatively with the kind of material,—and this means that a person may be relatively quick in one kind of decision and relatively slow in another. Such variations are, however, the exception. One subject, R, shows increase for every series, and Q and A, for all except the last, while C and M show decreases for every series except the post cards. It is interesting to note that the former are also the subjects that show the *increase* in decision time for the second presentation, as are the latter those that show the greatest decrease. This correlation is no doubt due to the greater adaptability of the latter.

The coefficient of variability (column C. V.) is obtained for each subject by dividing the mean variation of his times for the five series by his average time for the whole hundred decisions. While this coefficient is probably not so accurate as that which could be obtained by finding the mean variation for each subject's times for the *hundred* decisions, yet it is sufficient for our purposes. The most "uniform" subjects are M and A, who have C. V.'s of .12 and .13 respectively; and the most variable are N and C, with C. V.'s of .36 and .31. Subjects A and C were also "uniform" and "variable" subjects respectively in our first investigation; and we also note here, as we did then, that there is no correlation between a subject's variability in his own decision time and his variability from the average for all the subjects. This result is significant; for it is thus possible to obtain subjects who are not only quicker than the average but also more uniform in time. Variability in time is not correlated with decision time, constancy, nor suggestibility; but if we refer to Table XIV, we shall see that the subjects who have the greatest C. V., show as a whole the greatest influence of what we have there termed "Inertia." This is not surprising, for the "uniform" subjects are those that standardize their time by *easily* bringing into play in difficult situations large amounts of reserve energy; while the variable subjects either have not the extra energy to expend or find it difficult to force into activity; that is, their inertia is great.

TABLE XIX.

Subject	*A.T.*^{1} *(100)*	*C. (30)*	*G. (20)*	*Pict. (20)*	*P.C. (20)*	*W. (10)*	*C.V.*
R	9.1	6.9	8.1	8.8	10.5	15.7	.25
Q	10.8	8.1	9.2	12.1	12.6	12.3	.17
A	12.7	10.7	11.5	11.9	15.5	14.1	.13
S	13.0	11.6	11.4	17.8	16.0	11.4	.19 +
C	14.6	18.6	11.3	8.3	20.2	10.7	.31
P	17.0	14.1	12.5	16.7	25.2	19.1	.21
M	18.1	21.8	17.5	16.0	18.2	13.7	.12
L	20.8	18.9	14.3	21.1	26.1	28.4	.20
O	24.5	29.4	18.5	21.9	28.5	19.0	.19 —
N	32.8	26.6	20.1	36.2	57.9	20.5	.36
Mean Averages	17.3	16.7	13.4	17.1	23.1	16.5	

A.T.^{1} (100) = Average time for the first decision on 100 groups.
C. (30) = Time for thirty decisions on colors.
G. (20) = " " twenty " " geometrical figures.
Pict. (20) = " " " " " pictures.
P. C. (20) = " " " " " post cards.
W. (10) = " " ten " " words.
C. V. = Coefficient of variability.

We pass now to a comparison of the results of the different series in constancy and suggestibility. In Table XX, we have summarized these results, combining the "suggestion" and the "non-suggestion" groups. Column 1 contains the changes of each subject for the whole hundred decisions: those in which the suggestion was not accepted (or not given), those in which it was accepted, and those in which it was reversed. In the other columns are similar results for the colors, geometrical figures, pictures, post cards, and words. The constancy of the subjects is shown by the = numbers, and the influence of the suggestion by the + and — numbers. The subjects are arranged in the order of their constancy which is indicated by the = numbers for the total in column 1,

The totals for all the subjects at the foot of the table show that on the whole the constancy (as shown by the = numbers alone) is relatively about the same for each series. For the whole hundred decisions the number of = changes is 427; that is, about 43 for each ten. At this rate we would get 129 for the thirty colors, 86 each for the geometrical figures, pictures and post cards, and 43 for the words. *Omitting the last,* the pictures show the least number of changes and the post cards the most; but the differences are much less than we would expect. The individual subjects, however, show greater variations, and show them in different ways. These variations are brought out in Table XXI, where, omitting the words, the series in which each subject is most constant and that in which he is least constant are given. Subject A is about the same in each series.

TABLE XX

Subject	*(100) C=(+)C+*	*30 C.*	*20 G.*	*20 Pict.*	*20 P. C.*	*10 W.*
A	31=17+ 3—	8= 4+ 1—	6= 7+ 1—	7= 3+	7= 3+ 1—	3=
C	31=19+ 10—	9= 6+ 1—	2= 7+ 1—	3= 2+ 2—	13= 1+ 4—	4=3+ 2—
O	33=36+ 4—	11=15+ 1—	11= 7+	4= 8+	6= 5+ 2—	1=1+ 1—
P	39=30+ 1—	9= 9+	10= 6+	9= 3+	8= 8+ 1—	3=4+
L	43=35+ 9—	9=11+ 3—	15= 9+ 3—	5= 5+ 3—	11= 6+	3=4+
M	45=24+ 7—	19=10+ 1—	8= 3+ 3—	9= 4+	7= 4+ 3—	2=3+
N	45=14+ 5—	17= 7+ 1—	6= 3+ 3—	7= 1+	10= 3+ 1—	5=
S	50=22+	15= 5+	8= 8+	10= 4+	14= 3+	3=2+
R	52=38+ 4—	19= 9+ 1—	12=12+ 1—	9= 8+	9= 7+ 1—	3=2+ 1—
Q	58=44+ 4—	19=10+ 2—	10=11+ 1—	16=10+	9=11+	4=2+ 1—
Totals	427=279+ 47—	135=86+ 11—	88=73+ 13—	79=48+ 5—	94=51+13—	31=21+ 5—

(100) C=(+)C+ = The number of changes for the one hundred decisions,—non-suggestion and suggestion groups combined.
30 C. = As above for thirty decisions on colors.
20 G. = " " " twenty " " geometrical figures.
20 Pict. = " " " " " " pictures.
20 P. C. = " " " " " " post cards.
10 W. = " " " ten " " words.

TABLE XXI

Subject	*Constant*	*Inconstant*
A		
C	Pict. & Geom. Fig.	Post Cards
O	Pictures	Geometrical Figures
P	Colors	Geometrical Figures
L	Pictures	Geometrical Figures
M	Post Cards	Colors
N	Geometrical Figures	Colors
S	Geometrical Figures	Post Cards
R	Pict. & Post Cards	Colors
Q	Post Cards	Pictures

The influence of the suggestion is shown by the + numbers in Table XX; and, as explained above, the degree of suggestibility by the amount which the + numbers exceed half the = numbers. If we wish to compare the influence of suggestion in the various series, we must compare not the total numbers but their relation to the = numbers. Thus, for a total of 427 = numbers, there are 279 + numbers; that is, an excess of 66 over half the = numbers. The following are the estimated excesses at the same ratio and the actual ones:

	C.	*G. F.*	*Pic.*	*P. C.*	*W.*
Estimated	20.8	13.5	12.2	14.5	4.8
Actual	18.5	29.0	8.5	4.0	5.5

If the force of the suggestion were wearing off, we would expect a gradual decrease from colors to words; but the rate is really slightly greater for words than for colors; and we are, therefore, justified in saying that the very low figures for post cards and for pictures cannot be due to a diminution of the force of the suggestion. On the other hand by far the greatest suggestibility is shown in the case of geometrical figures. An explanation that suggests itself is that the decisions were on the whole more indifferent here; but, if inconstancy means indifference, then the post cards in that they show the most inconstancy are the most indifferent. Yet they show the least influence of suggestion. The greater influence of suggestion in the case of the geometrical figures must, therefore, remain unexplained.

Contra-suggestibility is shown by the — numbers, and here also their relation to the = changes must be considered for comparison. The following are the figures estimated from the ratio of 427= to 47—; and also the actual figures:

	C.	*G. F.*	*Pic.*	*P. C.*	*W.*
Estimated	15.0	9.7	8.7	10.4	3.4
Actual	11.0	13.0	5.0	13.0	5.0

The contra-suggestibility is large not only for the geometrical figures, which showed also the greatest effect of suggestibility, but also for the post card series, in which direct suggestibility was least. Contra-suggestibility is small for colors and pictures.

Many of the subjects show marked deviations from the general tendencies, so far considered. These variations are brought out better in Table XXII, where the series in which each subject shows the most suggestibility, the least suggestibility, and the greatest contra-suggestibility are given. The words are omitted. The individual differences here shown need not be further commented upon. An enumeration of the various series mentioned shows that there is most suggestibility for geometrical figures; and both least suggestibility and most contra-suggestibility for post cards. These results are in accord with those of W. D. Scott[1] and add weight to his view that suggestibility is not a general trait; but that there exist only varying degrees of specific *suggestibilities*.

TABLE XXII

Subject	*Most Sug.*	*Least Sug.*	*Contra-Sug.*
A	Geometrical Fig.	....	....
C	Geometrical Fig.	Post Cards	Post Cards & Pict.
O	Pictures	Geometrical Fig.	Post Cards
P	Colors & Post C.	Pictures	....
L	Colors	Post Cards	Pictures
M	Post Cards	Geometrical Fig.	Post Cards
N	Geometrical Fig.	Pictures	Geometrical Fig.
S	Geometrical Fig.	Post Cards	....
R	Geometrical Fig.	Colors	....
Q	Post Cards	Colors	....

The subjects were not asked for introspection upon the influence of the suggestion during the experiment; but very often it was given gratuitously. Of the suggestible subjects, R denies any influence, but Q, O, and P acknowledge it. Subject L, who is both positively and negatively suggestible acknowledges both tendencies in his introspection; S and A deny any effect of the suggestion; and the other subjects do not mention the fact at all. On the whole the introspection points to the correctness of our classification. At the end of the investigation the subjects were questioned regarding the influence of the "special arrangements," and whether they guessed the purpose of the experiment. Most of them said they did not think of the latter at all; and none of them ever thought they were being deceived regarding the card the majority had chosen as best and worst. All of them felt surprised that they should differ from the majority so often; but the introspection on the influence of the "special arrangement" differs from subject to subject,—in general, verifying our classification as to suggestibility. Some of the more suggestible subjects (e.g., O and P) said they often took the cards suggested because it was the easiest thing to do when a decision would be difficult. Subject M said he felt influenced against the suggested cards; and both C and N acknowledge considerable pride in differing from the majority; these three were classified as contra-suggestible. Subject S (non-suggestible) said he felt sometimes like defending the card the majority thought was *worst*. *A* knew more of the purpose of the experiment than the others; and his suggestibility is probably greater than calculated here. It is interesting to note that very often when a subject accepted the suggestions, he remarked that he remembered choosing the same card before, when of course he was really making a different choice.

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[1] W. D. Scott, Personal Differences in Suggestibility, Psych. Rev. 17, 1910, pp. 147-156.

The subjects were asked, especially in the first presentation, for introspection upon the decision itself; but this was not emphasized as the investigation has no direct bearing upon the introspective analysis of the "Act of Decision" itself. The material obtained, nevertheless, shows that the decisions were always brought about in one of the following ways: (1) it succeeds an immediate feeling attitude, which may be of various kinds but not further explicable; (2) the feeling is explicable,—

that is, the subject can tell *why* the object is pleasant or unpleasant, what factors in the situation produce it, etc.; (3) the feeling is aroused not by the object itself, but by something, more or less closely associated with it. The one invariable factor in these aesthetic decisions is the feeling. The nature of the feeling itself, the *kind of object* which produces it, and the kind of association which arouses it, all differ greatly from subject to subject. Many individual differences in this respect might be obtained by a careful study of the introspection and the nature of the objects chosen by each subject; but the results would scarcely repay the immense amount of work necessary to obtain them, especially as this is not an investigation in the psychology of aesthetics.

ACCURACY OF DECISION

The subjects were tested for accuracy of decision by the same method used in the last investigation. This time only cards containing from 44 to 55 holes were used for the comparisons; and the group areas were always the same, but the subjects were not informed of the latter fact. In the previous experiment we noted that certain illusions produced constant errors. In order to overcome this factor as far as possible, the subjects were always told whether their decisions were right or wrong, and were thus enabled to correct their judgments by experience. Two series of fifty decisions each were obtained. In one the time for each decision was taken with a stop watch; in the other a constant time of 5″ was given for each.

The results for both series are given in Table XXIII. There is a very great variation in time: from 2.6″ to 12.8″; and, contrary to our last investigation, there seems to be a pretty close correspondence between the times in the constancy of decision test, and those in the accuracy of decision test. In the Table the subjects are arranged according to time; and the first five here are, with one exception, the same as the first five in Table XVI, where they are arranged according to time in the constancy of decision test. This correlation may be due to the fact that the two kinds of decision are more alike in this investigation. Aesthetic decisions in which the *actual objects* are compared are probably more like the decisions we have required for accuracy than are the more logical decisions of the first investigation.

TABLE XXIII

Subject	*A.T.(50)*	*E.^{1}(50)*	*E.^{2}(50)*	*T.E.*	*C.V.*
A	2.6	18	19	37	.23
Q	4.0	23	19	42	.32
R	5.1	17	18	35	.31
M	5.2	18	18	36	.33
C	5.5	18	17	35	.27
O	6.3	22	15	37	.26
P	7.3	24	19	43	.44
L	7.9	17	18	35	.38
N	9.0	24	28	52	.56
S	12.8	18	12	30	.47
		199	183	382	

A.T. = Average time for fifty decisions.
E.^{1} = Errors for the fifty in which time was taken.
E.$_2$ = Errors for the fifty in which the time was constant (5″).
T.E. = Total errors.
C.V. = Coefficient of variability.

In column E.^{1} are the number of errors for each subject in the fifty decisions in which the time was taken; and in E.^{2} the number for those in which the time was a constant quantity (5″). The number of errors is on the whole less for the latter. This is probably not because the time was allotted, but because some of these decisions were really easier than in the other series. The difference between the number of holes in the two cards presented was usually from five to 0; but in twenty-five pairs of this series, it was from 10 to 0. Corresponding to this *objective* difference, there was a greater *subjective* ease in the decisions; for in these twenty-five decisions there were a total of 76 errors, while in the other twenty-five of the series there were 107. In the series in which the subjects were told to decide as quickly as possible and the time measured, the errors for the second twenty-five numbered 103. This series was the first given, and we should have expected a decrease in the number of errors through practice. Hence, the increase from 103 to 107 errors for the same twenty-five decisions shows that the allotment of a constant time (5″) on the whole *decreased* the accuracy. Of course the time given

(5″) hastened only the relatively slow subjects, while some of the quick ones were given extra time. The noticeable thing is that the *lengthening of the time for the quick subjects did not on the whole effect their accuracy, while the shortening of the time for the slow subjects seems to have, in some cases, actually increased their accuracy;* e.g., subjects O, P, and S, whose times are much over 5″, make fewer errors when compelled to decide in a hurry; N and L, however, make more. These statements are verified by the results for the twenty-five decisions that were the same in each series, as well as by Table XXIII. A possible explanation is that the most reliable judgments are based upon the immediate *feeling of difference,* that the time required for the development of this feeling is different with different subjects; and that, if left to themselves, some subjects tend to go by this feeling, while others over-rationalize and by trying to consider all the pros and cons in the situation greatly lengthen their time and on the whole decrease their accuracy. Such subjects do better if hurried; but of course there is also danger of over-hurrying; for if the time is so short that the feeling of difference has not completely developed, the errors may be again increased. All this is merely hypothetical, but the actual increase of accuracy with decrease of time observed in some cases adds weight to the view that there may be an *optimal judgment time.* V. A. C. Henmon says: "There may be an optimal time for judgment, varying with individuals and with varying stimuli and conditions, and judgments made too soon or too late are apt to be wrong."[2] This view could be experimentally tested by getting decisions on the same material with times longer and shorter than the time taken by each subject when left to himself.

The total number of errors in comparison vary from 30 for S to 52 for N. As before, we find no correlation between accuracy and time; and the four types,—quick accurate, quick inaccurate, slow accurate, and slow inaccurate, are, therefore, represented but not clearly demarcated.

As in the last investigation, we find some evidence of a correlation between quickness in subjective decisions (here aesthetic) and accuracy in objective; but this correlation is not as marked as before. It is shown by the most accurate and the most inaccurate subjects, but not by the intermediate. The most accurate subject in the list (S) is quick in subjective decisions and slow in objective; but capable of doing just as well if compelled to make the latter decisions more quickly. This subject is also the least suggestible. There is no perfect correlation brought out between suggestibility and accuracy; but three of the most suggestible subjects (Q, P, and O) are also inaccurate.

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[2] The relation of the time of a judgment to its accuracy, Psych. Rev. 18, 1911, pp. 186-201.

A coefficient of variability is obtained for each subject in the following day: The probable error is first found by arranging the decision times in order of magnitude, counting off one-fourth of the cases from each end of the series and halving the difference between the two values thus obtained.[3] This probable error for each subject is then divided by his average decision time. The slowest subjects here have the largest coefficients. This was not the case in aesthetic decisions. Moreover, there is no complete correlation between the coefficient of variability here and that for aesthetic decisions. Subjects S and M, who have very low coefficients in the latter, have large ones here; and R and C are just the reverse. The other six subjects are about the same *relatively;* but the C.V. is, on the whole, larger in this experiment. There is some appearance of a correlation between variability in decision time and inaccuracy. There are three very variable subjects; two of these are the most inaccurate in the list; while, curiously enough, the other (S) is the most accurate. On the whole, therefore, we may say that variability is here, as in the previous experiment, an independent factor, not *definitely* correlated with either time, accuracy, or suggestibility.

As in the previous similar experiment, each subject was asked to state his confidence as A, B, or C, in each decision; the meaning of these symbols and the method of evaluating them has been described above. In Table XXIV are given in numerical values, the total confidence of each subject for the hundred decisions, and his average confidence for correct decisions and for errors. The subjects are arranged in the order of their total confidence, with the most confident first. If this total confidence for each subject is compared with his number of errors in column T. E., the previous observation that "the *confident* subject is no more liable to be correct than the *inconfident*" is amply verified. In fact, two of the most confident subjects (P and N) are the two most inaccurate. The least confident subjects come next; and the subjects with *medium confidence* have the highest accuracy.

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[3] See Whipple's "Manual of physical and mental tests." page 18.

If now the average confidence for correct decisions be compared with that for errors, the former conclusion that a *confident judgment of any given subject is more liable to be correct than a less confident one by the same subject,* is also verified. The difference between the mean average confidence for correct (1.1) and for incorrect (.8) decisions is, in general, less than might be expected; but this difference is much greater for some subjects than for others. The most confident subjects show the least differences; and the greatest differences are shown by A, O, and L,—relatively inconfident subjects. In other words, a subject who is by nature very confident is just about as confident when he is wrong as when he is right; while one who by nature is of medium or little confidence varies greatly from his correct to his incorrect decisions. In the latter case the probability of being correct increases with the confidence; in the former it does not.

Henmon in his work on judgment studied the relation of confidence to accuracy, and concludes that "There is a positive correlation on the whole between degree of confidence and accuracy; but the degree of confidence is not a reliable index of accuracy."[4] This is in perfect accord with our finding that the confident judgment of a given subject is more liable to be correct than an inconfident one by the same subject. Henmon, however, did not distinguish between confident and inconfident subjects; but only between confident and inconfident judgments; and, therefore, his conclusion is quite compatible with the statement made above that the confident subject is not more apt to be correct than the inconfident.

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[4] Ibid. Page 200.

TABLE XXIV

Subject	*T.C.*	*A.C.C.*	*A.C.E.*	*T.E.*
C	146	1.5	1.3	35
P	139	1.5	1.3	43
N	125	1.4	1.1	52
R	93	1.0	.8	35
S	92	1.0	.7	30
L	86	1.0	.5	35
Q	86	1.0	.6	42
A	77	1.0	.3	37
M	76	.8	.7	36
O	72	.9	.4	37
Average Con. for each Dec.	99	1.1	.8	

T.C. = Total confidence.
A.C.C. = Average confidence for correct decisions.
A.C.E. = Average confidence for errors.
T.E. = Total errors in accuracy of decision test.

SITUATION TEST

In connection with the experiment on accuracy of decision, it will be well to state the results for the "Situation Test" (or A, E, O, U Test). This test was originated by Professor Münsterberg in the interest of the ship service; and is described in his "Psychology and Industrial Efficiency."[5] It aims to determine reliability of judgment in an emergency; and, therefore, like the experiment just reported tests degree of accuracy and rapidity in decisions. The comparison of the number of holes in two cards is, however, a relatively simple decision, while the A, E, O, U Test introduces a much more complex situation. There are more factors to be eliminated and the experience is more *baffling* than in the comparison test. The latter may probably be considered as a test for accuracy and rapidity of decision in ordinary, everyday life, while the former tests the same factors in an emergency. Of course, these two traits may or may not be correlated. It would not be unreasonable to suppose that many people who are accurate and quick in ordinary life affairs would lose their heads completely in an emergency. We shall, therefore, not be surprised if the results of these two tests are not identical.

The results are given in Table XXV. The product of the time multiplied by the number of errors in our experiment on

[5] Pp. 83-96.

accuracy of decision is also given for comparison. As expected, there is not a close correspondence; but the largest products in both experiments are in the lower half of the columns and the smallest in the upper. There are four exceptions to this: L, O, A and R. None of the ten subjects have products less than 1661 in the situation test, while according to the author of the test, the perfectly reliable should make a product of 400 or less. The importance of considering not only the product but also the time and the number of mistakes in estimating any individual is well illustrated by this table. Subject O, e.g., who has the fewest mistakes, is disqualified by the great length of time; and Q, who has the shortest time, is disqualified by the number of errors. On the whole, neither the products just considered, nor the number of errors, nor the time in this test corresponds completely to the same factors in the comparison test.

TABLE XXV

	Situation Test II				
Subject	*Product*	*Time*	*Mistakes*	*Test I*	*T x E in C*
L	1661	151″	11	18.2″	276.5
Q	1890	105	18	21.2	168.0
M*	2040	120	17	20.0	187.2
O	2130	355	6	23.0	233.1
C	2190	146	15	15.4	192.5
A*	2300	230	10	16.4	96.2
S	2440	244	10	16.4	384.0
P*	2646	126	21	18.2	333.9
R	2814	134	21	24.0	178.5
N	3300	150	22	41.6	468.0

Product = Time x number of mistakes.
Test I = Time for sorting the cards into four groups,—A, E, O, U.
T x E in C = Time x errors in experiment on comparison of number of holes.
* = Subjects who had taken the test before.

If the number of errors alone be considered, an interesting correlation between them and suggestibility is revealed. This is shown in Table XXVI. It will be seen that with one exception the non-suggestible make the fewest errors; those who are both positively and negatively suggestible come next; and the most mistakes are made by those who are positively suggestible alone or negatively suggestible alone (N). This correlation is not unexpected, when the actual experience in performing the test is considered. This shows that when a card is selected as having more of any given letter, there is a tendency to look for and hence find the same letter predominating in the next card. This tendency is greater for and not so easily overcome by the suggestible subjects. Subject O's exceptional position can be explained by supposing that she was able to overcome the force of this suggestion by greatly prolonging the time. The situation test thus becomes a good test for a certain kind of suggestibility; but this may be no objection to it as a test for reliability in emergency; for those who lose their heads in an emergency may always be suggestible people.

This correlation with suggestibility is shown but not so well by the errors in the comparison test (Table XXVI). We have remarked in Chapter I that this test brings out a liability to illusions, which is probably similar to the kind of suggestibility brought out by the "Situation Test." This liability to illusions tended to produce constant errors there; but in the later similar experiment this tendency was partly overcome by informing the subject of the correctness or incorrectness of each decision. Thus another factor was introduced which no doubt affected the results considerably.

TABLE XXVI

Subject	*E.S.*	*E.C.*	
O	6	37	Suggestible
S	10	30	Non-suggestible
A	10	37	
L	11	35	Suggestible and Contra-Sug.
C	15	35	
M	17	36	
Q	18	42	Suggestible
P	21	43	
R	21	35	
N	22	52	Contra-Suggestible

E.S. = Errors in "Situation Test."
E.C. = Errors in Comparison Test.

The method of evaluating the mistakes in the situation test deserves some consideration. In some cards the predominating letter occurs twenty-one times, in others eighteen times, in others sixteen times and in still others fifteen times, out of a total of forty-eight. Hence, a mistake in the first card is much more serious than in the last, and this must be taken account of in evaluating the results. The method adopted was to count the mistakes in these cards as 4, 3, 2 and 1 respectively, on the assumption that their difficulty varied inversely as these numbers. Now this is the assumption that should be further investigated. It might be done by giving the test to a large number of people, and then estimating the difficulty of the respective cards by the total number of mistakes made in each. Such a record was kept of the ten subjects and the results are shown in Table XXVII. On the whole, the number of mistakes made in the 21, 18, 16, and 15 cards were 3, 30, 17, and 28 respectively; but since there are twice as many 18 and 16 cards as 21 and 15, the figures for equal numbers of cards would be 3, 15, 8½ and 28. Thus the 18 cards are really more difficult than the 16, since there were more mistakes made in them. This seeming paradox is explained by the fact that in the 16 cards a number of consonants are intermixed with the vowels; and, since the total number of letters is the same, the number of vowels from which the predominating one is to be selected is less, and thus the selection easier. Therefore, in making the calculations for Tables XXV and XXVI, mistakes in 16 cards have been valued at 3, those in 18 at 2, instead of the opposite. This, however, is only a provisional correction, and the results for many more subjects should be considered to obtain an accurate value for each card.

Supposing such a value were obtained, it might still be rendered invalid for some cases by another circumstance: viz., individual differences as to what card is found most difficult. The presence of consonants in the 16 cards may, in some cases, have the effect of increasing instead of decreasing the difficulty. This seems to have been actually the result in the case of subject P, who made 4 mistakes in 16 cards, to 1 in 18. For some subjects again, the subjective difficulty may not increase in proportion to the objective differences; for others it may. If on further investigation such individual differences turn out to be very great, the only way of evaluating the mistakes would be for each subject on the basis of his own errors; and then comparison of one subject with another would be difficult or impossible. Other individual differences occur in the letters which are found most difficult. Cards in which the O or U predominated were usually found easier than those in which A or E predominated; but for some subjects the opposite might be true. Such differences, however, would not make any difference in the method of evaluating the mistakes; for the numbers are equally distributed among all the letters.

TABLE XXVII

Subject	*21*	*18*	*16*	*15*	*Total*
O	0	1	0	4	5
L	0	3	1	2	6
A	0	4	0	2	6
S	0	2	1	3	6
M	1	3	2	1	7
C	0	3	2	3	8
P	1	1	4	3	9
Q	0	3	3	3	9
R	0	4	3	4	11
N	1	6	1	3	11
	3	30	17	28	
	3	15	8½	28	

ASSOCIATION TEST

The subjects were tested for association time as in the first investigation. In all, forty reactions were obtained for each subject, and the time taken with a stop watch as before. The results are given in Table XXVIII. The times for the aesthetic decisions, and for the accuracy of decision test are also given for comparison. The correlation between aesthetic decision time and association time is more marked than that shown between "logical" decision time and association time in the last investigation. If the method of unlike signs, which considers only the nature (+ or —) of the deviation of the individual subjects from the central tendency and not its amount, be again applied here, it will be found that there are only two cases (L and Q) of unlike deviation,—that is, 20% cases of unlike signs; and this signifies, according to Whipple,[6] a correlation of about .8 between association time and aesthetic decision time. The correlation between time for comparison of the number of holes in two cards and association time is, however, less marked,—there being four cases of unlike signs. The time for sorting cards, as obtained in Situation Test I, follows the association time pretty well. The only marked exception is subject R, who was not used to handling cards and felt awkward about it. Subject N's time was greatly lengthened for the same reason. This is the nearest approach we have to a reaction time test; and the indications from it are that reaction time would be directly correlated with decision time.

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[6] "Manual of physical and mental tests," page 40.

TABLE XXVIII

Subject	*A.*	*M.*	*A.D.T.*	*C.T.*	*T.S.C.*
C	.94	1.0	14.6	5.5	15.4
S	.96	1.0	13.0	12.8	16.4
R	1.04	1.0	9.1	5.1	*24.0
A	1.06	1.0	12.7	2.6	16.4
L	1.09	1.0	20.8	7.9	18.2
P	1.24	1.2	17.0	7.3	18.2
N	1.29	1.3	32.8	9.0	*41.6
Q	1.32	1.2	10.8	4.0	21.2
O	1.35	1.2	24.5	6.3	23.0
M	1.61	1.5	18.1	5.2	20.0

A. = Average association time.
M. = Median association time.
A.D.T. = Time for aesthetic decisions.
C.T. = Time for comparison of the number of holes.
T.S.C. = Time for sorting cards, as shown in Situation Test I.
*. = Not used to handling cards—felt awkward about it.

In the last investigation, a correlation between quickness of association time and constancy in decision was brought out (see page 15). This provisional conclusion is not verified by the results of this experiment; but it must be remembered that the decisions were upon different material, and the results for constancy not so satisfactory and definite as in the previous case. Then there were many more experiments to obtain a constancy index and the method employed—arranging the whole five cards in order of preferment—is a much subtler means of bringing out the relative constancy of the subjects; but, of course, not so well adapted to the work on suggestibility as the one used in the second instance. The result here cannot, therefore, be considered as a refutation of the previous.

If now we compare the association times with the errors in the accuracy of decision test (Table XXIII), we shall find that the quick subjects are, on the whole, the most accurate. There are only two cases of unlike deviation. It would seem then that, though quickness of association time does not surely guarantee quickness in the comparison of the number of holes in the cards, it does point more certainly to accuracy in this test. It may very well be that inaccuracy in the comparison test and slow association time depend upon a common factor; and this factor may be inhibition, delayed or erroneous apperception, or some other of the many factors that lengthen association time. We know from our association experiment that the slow subjects had many associations as quick as those of the quickest subjects, but reported many more obstructions, inhibitions, etc., so that these account, in part, at any rate, for the longer times. Such inhibitions and other irregularities occurred most frequently with subjects O and Q; to a considerable extent with M, N, P, and L; and almost not at all with the other subjects.

MEMORY TESTS

The method of testing the memory of the subjects was slightly different from that used in the previous investigation. The material was again figures, nonsense syllables, and words. Memory for figures was tested as before, except that the subject repeated each number from memory immediately instead of after a five-second interval. In testing memory for nonsense syllables and words, the Lipmann-Marx memory apparatus, which exposes the words successively at a very regular rate, was made use of. Lists of twelve nonsense syllables, and of eighteen unassociated words were constructed; and immediate memory efficiency or learning ability was measured by the number of repetitions required to memorize these lists. The memory was taken as complete when the subject could anticipate one word or syllable ahead throughout the list. In the case of the words the subjects were told to use artificial associations to help them in memorizing; but to prevent these as far as possible with the nonsense syllables. The object of this was to obtain a measure of association strength in the former case, as well as receptivity in the latter. Having learned a list of nonsense syllables and a list of words, the subject was occupied for 25 minutes in the accuracy of decision test described above; and then asked to write down first all the nonsense syllables and then all the words he still remembered. His memory for a period of 25 minutes was thus measured by the number of words and syllables he still remembered. A test of memory for figures, for words, and for nonsense syllables was given each subject every week for three weeks; and the average efficiency for the three tests taken as the measure of the subject's memory ability for each kind of material.

We shall first consider the memory span as measured by the number of digits reproduced immediately after two repetitions. Every subject showed improvement with practice in this test; and some were able to reproduce one figure more at each test than at the last; but only the averages for the three tests are given in Table XXIX. These results partly verify our conclusion regarding memory span in the last investigation. As before, there is no correlation between memory span and decision time for either kind of decision; but there seems to be a correlation, not so marked as before, between memory span and constancy,—for the most inconstant have poor memory spans. Moreover, there is here shown a considerable correlation between accuracy of decision and memory span. There is probably only one exception (subject R), and this would mean a correlation coefficient of .9 or over. We have observed above that there is probably a correlation between suggestibility and inaccuracy. If this is so, then suggestibility, inaccuracy, and poor memory span ought to go together. The fact that the four most positively suggestible subjects have also the poorest memory spans affords some confirmation of this supposition. Since memory span for figures is closely related to span of attention, and a broad span of attention means the ability to take in all the details of a situation, which is essential to correct evaluation and decision, we would naturally expect this correlation between memory span and accuracy of decision. The accuracy spoken of here, as elsewhere in the discussion of memory, is for the comparison of the number of holes in pairs of cards, and not for the situation test. The results for the latter show less evidence of correlation with memory in every instance.

TABLE XXIX

Subject	*M.S.*	*C.*	*A.*
L	11⅓	35	35
C	11	21	35
S	10	39	30
M	9⅔	30	36
N	9⅓	37	52
A	9⅓	21	37
Q	9	43	42
P	9	32	43
R	9	43	35
O	8	29	37

M.S. = Memory span for figures.
C. = Constancy, number of changes in 50 aesthetic decisions.
A. = Accuracy, number of errors in 100 decisions.

The immediate memory or learning ability of the subjects, as measured by the number of repetitions required to memorize the lists, is shown in Table XXX. The average number of repetitions for three lists of twelve nonsense syllables each, and for three lists of eighteen words each is given for each subject; and then these figures are added to get an idex of learning ability. The number of repetitions for words and those for nonsense syllables correspond, on the whole, pretty well. The largest figures for each are in the lower halves of the columns; but there are some important variations. Some of the subjects were accustomed to work with nonsense syllables, and, therefore, had some slight advantage; these are marked with an asterisk. No correlation is brought out between learning ability and decision time, constancy or accuracy, except in the case of repetition for words taken alone. Here a slight correlation between quick learning and accuracy is shown, but subject N is a marked exception—very inaccurate, yet learning very quickly. The correlation mentioned is important, for in this test the subjects were told to make use of artificial association to aid them in learning. It means, therefore, that rapidity in the formation of associations is correlated with accuracy, while mere receptivity, as tested by nonsense syllables, is not; and this is a similar correlation to that brought out in the association test where it was found that quickness of association time was correlated with accuracy of decision. There is, therefore, no doubt that the speed and certainty of association is an important factor in assuring accuracy in decision. Learning ability for nonsense syllables is not at all correlated with memory span for figures; but, as expected from their mutual correlation with accuracy, the learning of words is. There are only two cases of unlike deviation (signs): subjects C and R, the former good in memory span and poor in learning,—the latter the opposite.

TABLE XXX

Subject	*L.*	*W.*	*N.*	*E.A.*
R	13⅓	5⅔	7⅔	35
*N	16	7	9	52
*O	16⅔	10	6⅔	37
M	17⅓	6⅔	10⅔	36
*L	18⅔	7⅔	11	35
P	19⅔	9	10⅔	43
S	24⅔	8⅓	16⅓	30
*A	26⅔	11⅓	15⅓	37
C	28⅓	13⅔	14⅔	35
Q	32	16	16	43

W. = Number of repetitions for lists of eighteen words.
N. = Number of repetitions for lists of twelve nonsense syllables.
L. = Learning ability, obtained by adding the two above.
E.A. = Errors in accuracy of decision test for comparison with column W.
* = Subjects who had previous experience with nonsense syllables.

The memory of the subjects was also measured by the number of words and syllables remembered after a period of twenty-five minutes. The results (given in Table XXXI) flatly contradict the correlation noticed in the last investigation between memory over a period of one week and rapidity of decision. There it was observed that the subjects quick in decision had better memories than the slow. Here, on the contrary, the slow subjects have, with only one exception, better memories than the quick. Of course, both results may be true since one experiment was on memory for a week and the other on memory for twenty-five minutes; and a subject who remembers well for twenty-five minutes may not remember relatively as well for a week.

As before, no definite correlation is brought out between memory and constancy or accuracy. The two subjects, however, who have the best memory for words (L and S) are also accurate subjects. We would expect from previous considerations that strength of association (memory for words) would show some correlation with accuracy, and that retentiveness, as tested by memory for nonsense syllables, would not. We would expect also that memory for words would correspond pretty well with memory span for figures; and this is true, with the exception of subject C, who is very good in the latter and very poor in the former.

In tests for memory it is very difficult to keep the conditions the same for all subjects, especially where, as in this case, the subjective interests of the subjects are not the same. Each subject does best in the test in which he is most interested or likes best. This is true of any test; but it is probably not so serious an objection as might be supposed for the interest may follow the ability instead of the reverse as is usually supposed. Objective conditions were always kept as near as possible the same. The subjects were asked not to think of the material learned during the 25-minute interval, in which they were occupied, as stated above, in the accuracy of decision test. In the first test they did not know they were going to be asked to write what they could remember after 25 minutes; and, as the results show that they did as well in it as afterwards, we conclude that they succeeded in following the instructions in the second and third tests.

TABLE XXXI

Subject	*W.*	*N.*
L	18	10⅔
S	18	9⅔
N	17	9⅓
M	17	9⅓
P	15⅔	9⅓
A	15⅔	8⅓
R	15⅓	8
Q	15⅓	7
O	14⅔	6
C	9	3

W. = Number remembered out of eighteen words after 25′.
N. = Number remembered out of twelve nonsense syllables after 25′.

ATTENTION

The relation of attention to decision time, constancy and accuracy is probably of more importance than any of the relations so far considered; but there are as yet no adequate tests for attention and the work on this point is therefore even more meager than on the others. *Span* of attention is probably fairly well tested by the work on memory span for figures. Two other tests were now made: the cancellation test (A-Test) for *duration* of attention, and the dot-counting test for concentration of attention.[7] Both of these tests involve other factors as well as attention; but there can probably never be a test for attention alone; for attention means attention to something and therefore involves at least sensory and perceptual factors.

The A-Test is chiefly a test of visual perception and recognition. It, of course, requires attention, but the results are ambiguous. A poor showing may be due to defects of perception or slow reaction rather than poor attention. Each subject was given only one test. A page containing thirty lines of pied text was used. There were 51 A's to be cancelled. Efficiency was measured by the time taken to perform the task and the number of errors. Whipple gives the formula $A = \frac{C - W}{C + O}$ to compute the index of accuracy.[8] C = the number of letters crossed; W = the number wrongly crossed; and O = the number erroneously omitted. Since none of the ten subjects had any of the W class, the number of letters crossed out of 51 can always be taken as the index of accuracy. Also, to compute a single index of efficiency, Whipple gives the formula $E = \frac{S}{A}$, in which E = the desired efficiency, S = the time, and A = the accuracy index; but, since the accuracy varied so little and the time so much, the efficiency as computed by this formula corresponds exactly to the time, which we have therefore used in our statement of the results in Table XXXII.

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[7] For description of these tests see Whipple's Manual, pp. 254-273.
[8] Ibid. pp. 260-261.

TABLE XXXII

Subject	*Time*	*A.*	*S.T.*
L	110″	47	1661
P	131	50	2646
M	136	51	2040
A	160	51	2300
Q	162	50	1890
S	172	51	2440
O	177	51	2130
C	191	49	2190
N	227	50	3300
R	252	47	2814

A. = Number cancelled out of fifty-one.
S. T. = Products in the situation test.

These results show no relation to time, constancy or accuracy in the decision experiments, but they correspond fairly well with the products in the situation test. This is probably because in both tests the perception and recognition of letters are important factors. There is probably also, as in the situation test, some relation to suggestibility; for the mistakes are all made by suggestible or contra-suggestible subjects.

The dot-counting test also involves other factors than attention; and these are, according to the introspection of the subjects, chiefly visual schematism or ingenuity in grouping the dots and immediate memory for these groups. Each subject was given two tests; in the first there were 50 dots, in the second 57. The errors were counted by taking the difference between the given and the true numbers. The total errors and the average time for the two tests are given for each subject in Table XXXIII. The O or U signifies that the mistakes were made in over- or underestimating the true number. There seems to be no correspondence between time for dot-counting and time for the cancellation test. In fact, the quickest in the latter was the slowest in the former; but there is not a general inverse relation. There is also no relation brought out between the results for the dot-counting test and time, constancy, or accuracy in the decision test; nor is there any relation, as in cancellation, to the situation test. On the whole, however, the least suggestible subjects are the quickest in dot-counting, though not always the most accurate.

These tests may probably have been successful in showing the relation of the factors tested to decision type; but it seems to us that attention was not the most important of these factors, and that the results cannot be considered as any indication of the relation of attention to decision. In all probability attention is a much more significant and crucial factor at least in accuracy than these results would show; and its real place is more likely better revealed by the tests for memory span.

TABLE XXXIII

Subject	*Time*	*E.*
S	16.5′	1—O
M	20.0	3—U
A	21.0	5—U
N	22.0	2—U
O	25.5	1—U
R	36.5	7—U
Q	37.0	1—U
P	53.5	1—O
C	63.5	1—U
L	78.0	2—O

E. = Errors for two tests.
O. = Over-estimations.
U. = Under-estimations.

ORIGINALITY TEST

The last test we have to report had for its object to bring out the originality or inventiveness of the subjects. The following method, suggested by a paper of Professor Royce's on the "Psychology of Invention,"[9] was adopted: First a circle was presented to the subject, who was told to draw another geometrical figure as different as possible from this; then a rhombus with diagonal was presented and the subject asked to draw *anything else,* as different as possible; and finally five concepts were given one at a time and the subject was to write other concepts as different as possible from these. The degree of originality is shown by the ability to break away from the impressions or ideas, and by the time of the performance. The tendency in the first test would be to draw a square, in the second to draw some other geometrical figure, and in the third to write the opposite of the given concepts, which were: mind, cause, substance, man, and heaven. If this tendency were followed out, it would indicate a routine mind, incapable of breaking away from the dominance of the ideas and impressions borne in upon it; while the further it were departed from, the greater the degree of originality. Since the ordinary associations of these perceptions and concepts may not be exactly the same for every subject, as the ones mentioned above, it was necessary to take the introspection of the subjects into consideration in estimating the amount of departure from the ordinary. Of course, this estimation was no doubt also influenced by the subjective prejudices of the experimenter; but Table XXXIV gives the relative standing of the subjects as near as it could be estimated from the records. The subjects are arranged in order of originality with the most original first.

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[9] Psych. Rev., Vol. V, 1898, pp. 113-144.

TABLE XXXIV

Subject	*O.*	*Time*
L	16	228″
S	15	43
A	11	107
N	10	84
M	10	61
C	10	64
O	10	60
P	8	56
Q	4	87
R	2	48

O. = Index of originality.
Time = Total for all the tests.

The originality index was calculated in the following way: a complete departure from the above-mentioned ordinary tendency, shown by both the introspection and the objective records, was in Tests 1 and 2 valued at 5; a complete submission to it at 0; and various degrees of success at overcoming it were represented by values from 0 to 5. In Test 3, thorough success in getting away from the usual ideas was valued at 10, 2 for each concept. The figures given in column "O" of the table are, therefore, the values attained out of a possible 20. In general, those who were original with the impressions in Tests 1 and 2 were also original with the ideas in Test 3; and we, therefore, give only the total for all tests in the table. The same can be said of the time, and for the same reason only the total time for the three tests is given.

As we anticipated, the results in this test are inversely related to the results previously obtained for suggestibility. The most original are the least suggestible; next in order come those who are also contra-suggestible; and least of all the positively suggestible. There is only one exception to this: Subject L, who has shown the greatest originality, is both positively and negatively suggestible in the decision test; but his originality was only attained by the sacrifice of a great deal of time; while S, the least suggestible subject, is both quick and original in this test. Of course, we might follow our method in the decision test, and say that there are both quick and slow original, and quick and slow non-original types; for it is hardly fair to say that difficulty in overcoming the tendency, as shown by a lengthening of the time, if rewarded by final success, indicates a lack of originality. It is surely one type of originality—the originality of the thoughtful, persevering man as distinguished from that of the genius. This difficulty, however, shows some suggestibility, and if the test is taken merely as a test for suggestibility, the lengthening of the time must certainly be taken into consideration as a mark of that trait, for it appears that a suggestible person may be original but cannot be quick in his originality. If the time as well as the work done is thus evaluated, this test for originality becomes truly a very good test for suggestibility. The suggestible subject will succumb to the suggestion of the ordinary association with the idea or impression given, or will take *a very long time* to overcome it; while the non-suggestible will quickly break away from it.

Other correlations with originality are just the inverse of those previously noticed with suggestibility. Hence, the most original subjects are the most accurate in the comparison test and have the fewest mistakes in the situation test. This correlation is strengthened by subject L's position at the head of the list in the originality test.

All the tests given were very successful in bringing out large individual differences; but too few subjects were examined to obtain any adequate information regarding the correlation of the various traits. Several correlations have been hinted at; but the results in this respect were largely negative. It may be that on further investigation with a larger number of subjects correlations may be obtained where we have found none; and those we have suggested may be proved erroneous; but until refuted by more efficient investigations, we shall assume that these results point in the right direction. They seem to verify two important statements made by Thorndike[10]: (1) that the variations in mental traits are continuous and cluster around one central tendency or type; and (2) that in mental traits correlation and not compensation is the rule,—that is to say, efficiency in one trait is very likely though not certain to be correlated with efficiency and not deficiency in another.

[10] Educational Psychology, 1910, chapter X.

CONCLUSIONS

The very nature of the investigation renders this paper extremely difficult to summarize; but we shall endeavor to select from the results scattered throughout a few conclusions that may be regarded as of chief importance.

(1) There is no correlation between time and constancy in either logical or aesthetic decisions. Hence, we are able to speak of four decision types: quick constant, slow constant, quick inconstant, and slow inconstant. These types, however, grade into each other, so that there is no natural dividing lines between quick and slow, and between constant and inconstant.

(2) There is no correlation between time and *accuracy* in objective decisions, so that here also any time can be combined with any degree of accuracy. This fact has some practical significance; for it shows that subjects both quick and accurate exist. They are, however, probably rarer than the mediocre in time and accuracy, and their rarity no doubt increases with their efficiency.

(3) There is no correlation between time in subjective and time in objective decision, nor between constancy in the former and accuracy in the latter. That is to say, the consistent subject may be consistently inaccurate; and the subject slow in matters of taste or opinion may be quick in objective situations. There seems, however, to be some evidence of a correlation between quickness in subjective decisions and accuracy in objective,—a correlation probably based upon the concentration of attention required in both traits; but it is not sufficiently marked to warrant serious discussion.

(4) Some subjects who come under the quick constant class in simple decisions, change their type under more complex conditions, becoming slow or inconstant or both. If this change of type also occurs for time and accuracy in objective decisions (and the results for the comparison and situation tests seem to show that it does), the fact is of considerable importance; for the individual thus changing would be successful in vocations requiring simple

acts of decision, but would fail in any calling that demanded decisions in relatively complex situations.

(5) Subjects differ greatly in the variability of their decision times; so that there are "uniform" and "variable" subjects; but the variability in the decision times of any given subject has no relation to his variability from the mean average time for all the subjects. Hence, subjects can be found who are both much quicker than the average and also "uniform." The subjects "variable" in time are as a rule the same ones that change their type under more complex conditions. The coefficient of variability is greater in objective than in aesthetic decisions, and there is no correlation between the C.V.'s in the two tests. The C.V. is an independent factor, not correlated with time, constancy, or accuracy in decision.

(6) In general, practice has no marked effect upon decision times; but there are individual differences in this respect—some of the more adaptable subjects quicken the time.

(7) The decision time for the second presentation is, as a rule, less than for the first; but for some subjects it is relatively very much less than for others. These subjects probably learn very quickly from experience, and their efficiency increases rapidly with repetition; but their efficiency in novel situations as shown by the time for the first presentation is not altered. This is important practically; for such subjects would be successful in an occupation presenting very few situations with routine decisions in each, but might fail in any vocation which demands quick decisions upon always novel situations.

(8) In subjective decisions the confident subject is more apt to be constant than the inconfident; but in objective decisions the confident subject is not more liable to be correct. In the latter the medium subject has the greatest accuracy, while the over-confident and under-confident are both inaccurate. The over-confident, however, varies little in his confidence from right to wrong judgments, while the under-confident are much more confident for their correct than for their incorrect decisions. In both subjective and objective decisions the confident judgment of any given subject is more apt to be constant and accurate respectively than

his inconfident judgment; but the difference is less than might be expected.

(9) In subjective decisions there was more inconstancy with judgments which the subjects described as difficult. The same thing is shown by the fact that there were more changes in decisions where times were above the median, and hence more difficult, than in those below it.

(10) The subjects differ relatively in constancy with the different materials used in the aesthetic decisions. On the whole, there are most changes with the post cards and least with the pictures. They also differ in respect to the material which they find most difficult to decide upon. This is shown by their relative differences in *time* with the different materials.

(11) The accuracy of the objective decisions remained on the whole about the same when the time was allotted. Of the slow subjects, some were more inaccurate when hurried, while others actually did better; and the longer time did not increase the accuracy of the quick subjects. These facts may favor the theory of an "optimal judgment time"; or it may be that the slow who were more accurate when hurried belong to that class of individuals who, while quite phlegmatic in ordinary life, excel when called upon to fill an emergency.

(12) The general effects of suggestion upon decisions are to lengthen the time and increase the inconstancy; but these effects are much more pronounced with some subjects than with others. The suggestion also reduces the variability of the subjects from their central tendency in time and increases it in the number of changes.

(13) Subjects can be roughly classified into four types on the basis of their suggestibility: (1) the non-suggestible, (2) the positively suggestible, (3) the positively and negatively suggestible, and (4) the negatively suggestible. These types merely signify certain degrees of the trait; for there is probably no absolutely non-suggestible subject, just as there is no purely positively or purely negatively suggestible person.

(14) There is a correlation between suggestibility and accuracy. The less the suggestibility, the greater the probability of

accuracy. This is shown by the number of errors in the comparison test, as well as by the number of mistakes in the "situation test."

(15) The results for the "comparison test" and the "*situation* test" are by no means identical. The latter presents a more complex situation; and subjects who are equally excellent in simple decisions may be quite different in more complex ones.

(16) The situation test seems to be successful in bringing out the mental trait intended; but the method of evaluating the results and comparing the subjects requires further study and correction.

(17) The best material for testing suggestibility appears to be geometrical figures; for here the suggestion was most readily accepted. The post cards were the poorest material for bringing out suggestibility, but the best for contra-suggestibility. There were, however, marked individual differences as to the material which brought out the most or least of these traits.

(18) The suggestible subject *may* or may not be introspectively aware of his suggestibility. He may think his decision is unbiased or even remember that he decided the same way before, while his memory plays him false and he is really accepting the suggestion.

(19) Inertia, or the tendency to leave things as they are, exerts some influence on decision. This inertia is much greater with some subjects than with others. There is also some indication of its correlation with *variability* in time.

(20) Position also influences decision. This influence is one kind of suggestion, and of course varies in amount with different subjects. There is also considerable variation in the position that is found most attractive. Position B,—that is, one from the right, is the most favored.

(21) Association time shows some slight correlation with time in aesthetic decision; and also with *accuracy* in objective decision; in that the quickest in association time are the more accurate.

(22) In conformity with the above, we find that there is a correlation between rapidity of learning lists of words and accuracy; but not between the learning of nonsense syllables and accuracy. In the former, association is the chief factor; while in

the latter, receptivity or plasticity is a more important factor than association.

(23) Memory span for figures is also well correlated with accuracy and very slightly with constancy. It is also correlated with suggestibility,—the most suggestible having the poorest memory spans.

(24) Memory over periods of time, and time for sorting cards show no definite correlation to decision time, constancy, or accuracy.

(25) The results of the A-test are correlated fairly well with the products in the situation test; but neither the A-test nor the dot-counting test brought out any correlation with decision time, constancy, or accuracy. The least suggestible, however, make the fewest mistakes in the A-test; and are also the quickest, though not the most accurate, in the dot-counting test.

(26) The most original subjects are the least suggestible; but a suggestible person may be original, if he has the perseverance and will to overcome the suggestion; and this probably requires some contra-suggestibility.

(27) The most original subjects are the most accurate in the comparison test, and have the fewest errors in the situation test.

Vol. XVII
No. 2
PSYCHOLOGICAL REVIEW PUBLICATIONS
Sept., 1914
Whole No. 73

THE
Psychological Monographs

EDITED BY

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF HARVARD UNIVERSITY

The Genetic Aspect of Consonance and Dissonance

By

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PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.);
LEIPZIG (Koenigstr., 37); PARIS (16 rue de Condé)

PRINCETON
UNIVERSITY
PRESS

ACKNOWLEDGMENT

It is with great pleasure that I acknowledge my especial indebtedness to Professor Münsterberg for many helpful suggestions in the preparation of the following paper. To Dr. Langfeld I also owe much, both for his valuable assistance as subject in my experiment and for his criticisms afterwards.

CHAPTER I

INTRODUCTION

It is perhaps more true of music than of any other art that the critics of one generation are the laughing stock of the next. The question of acceptable harmonic progressions has been a bone of contention between composer and listener from everlasting to everlasting. It matters not with what period of musical history we concern ourselves, there is always evidence that the great composers were being continually taken to task on the ground of impossible harmony. Even Mozart was attacked by Hans Georg Nägeli[1] for certain progressions in his "Jupiter" symphony; Beethoven became notorious for outraging the tender feelings of theorists of his day; fifty years later Wagner was struggling to persuade the world to endure harmonies which were to become shortly afterwards a chief resource for popular nights at the symphony. In our own time Strauss set out by appalling his hearers with a "wild experimentation" which a London critic described as "discordant for the mere sake of discord";[2] one by one he has converted his opponents until, as Newman says,[3] "Any one who now thinks Tod und Verklärung a tough nut to crack is looked upon as a hopeless conservative." But the end is not yet. More recently Sibelius has given concert-goers a start with his Symphony Number 4, of which W. J. Henderson writes: "He has swallowed the disjointed sequences, the chord of the minor second, and all the Chinese horrors of the forbidden fifths." Another critic remarks, "It is as inconsequential as the ravings of a drunken man." Doubtless the critics of Sibelius will be making their apologies ten years hence.

What is the ultimate meaning of all this? For the answer to this question, practical music will have to turn to theory, and especially to the theory of consonance. How far an answer may be expected from this quarter we shall see at the conclusion of the present discussion. But, first, we shall be obliged to review and, to a certain extent, to revise the theoretical situation.

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[1] Jahn, Life of Mozart, III, 34.

[2] Newman, Musical Studies, 290.

[3] Op. cit., 291.

CHAPTER II

THEORIES OF CONSONANCE

The problem of consonance is one that may be attacked from the various standpoints of mathematics, of psychology, and of physiology. The last-mentioned branch of consonance theory may, however, be eliminated from consideration here, since the little that it has at present to offer is of very questionable validity. Of the other two, it was the mathematical aspect that most concerned theorists from the time of Pythagoras to that of Helmholtz, whereas during the last fifty years attention has been given increasingly to the psychological aspect.

The mathematical standpoint is best represented by the philosophers Descartes, Leibnitz, and more especially Euler,[4] all of whom were impressed in true Pythagorean fashion by the general significance of number relations. The fact of greatest moment to them was the correspondence betwen the physical datum of greater or less simplicity in vibration ratios and the conscious experience of greater or less consonance. Thus for the 1:2 ratio we have the very consonant octave; for the 2:3 ratio the less consonant fifth; for the 4:5 the imperfect consonance of the major third; and so on to the harshest dissonances. According to Euler the human mind perceives the commensurability of pitch numbers as such, and derives pleasure from discovering the law of their arrangement.

These correspondences between the physical and the conscious are unquestionably facts of fundamental importance, but they leave the conscious phenomena themselves no better understood than before. Further progress in the minute observation of the mental facts was inhibited in these investigators by their strong bias for the delights of "arithmetical mysticism," as Helmholtz expressed it.[5] Such a psychology as they worked out was merely mathematics with a new name. The changed standpoint of the present day is voiced by Professor Max Meyer when he says,[6] "Neither the physicist nor the physiologist can prove by physical or physiological laws why we must enjoy certain combinations of tones. It is the psychologist's task to determine the asethetic laws which describe the subjective as well as the objective conditions of aesthetic enjoyment."

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[4] Helmholtz, Sensations of Tone, Ellis' translation, *229* ff.

[5] Sensations of Tone, *229*.

But when we have claimed that consonance is essentially a problem for psychology, we have not yet limited its method of treatment. As psychologists we must, of course, regard our given conscious phenomena as so many facts to be described in detail and related to each other wherever possible. But this may be done with only a minimum of reference to the past or the future. We may simply characterize present facts in terms of other present facts. On the other hand, our prevailing interest may cause us to regard our subject-matter as merely a phase in a course of development. We may seek to clarify existing conscious phenomena by reference to their probable mode of genesis. The importance of the latter method as complementary to the first is fully recognized in general psychology, but the literature on the subject of consonance betrays as yet a rather marked bias for the first. Any given experience of consonance is usually taken for granted, as given once for all, and the nature of its ancestry is too little considered as a possible source of new light. This one-sided viewpoint will become apparent as we examine briefly the four leading psychological theories of consonance, those of Helmholtz, of Lipps, of Krueger, and of Stumpf.

HELMHOLTZ

The present-day psychology of consonance and dissonance begins practically with Helmholtz. The views set forth in his "Tonempfindungen," while by no means new at every point, represent the first thorough-going effort to embrace all the facts under a consistent theory. So tenaciously has this theory held on that in spite of repeated attacks from a very considerable number of critics, such as Oettingen, Mach, Riemann, Engel, Melde, Lipps and Stumpf, it is even to-day the popularly understood explanation of consonance and dissonance. But Stumpf gave it, for the large majority of theorists at least, the final *coup de grâce* in the first chapter of his "Konsonanz und Dissonanz."[7] Suffice it here to give a brief statement of Helmholtz's position, and a summary of the objections which have seemed to make necessary its abandonment.

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[6] M. Meyer, Contributions to a Psychological Theory of Music. Missouri University Studies, 1901.

The psychological principles invoked by Helmholtz,[8] in explaining the difference between consonance and dissonance, were two: similarity and sensory roughness. Consonances are characterized positively by the similarity of two tones due to their possession of identical overtones, and negatively by the absence of perceptible beats. Dissonances are characterized positively by their rough sensory quality due to the beating overtones of the two clangs, and negatively by a dissimilarity due to the lack of identical overtones. This theory has the great advantage of simplicity in that it finds its criteria in fairly obvious characteristics of the sensations themselves, and not in some more obscure processes supposed to be aroused by the sensations. But its critics have raised well-nigh unanswerable objections at every point.[9] To the statement that dissonance is due to beating overtones, it is answered:

1. That dissonance can be produced without beats, as when two simple tones of 700 and 1000 vibrations are sounded together. Here there are no overtones, and the fundamentals give 300 beats, which is above the Helmholtzian limit of perceptibility, i.e., 132. By putting together a number of such intervals, it is possible to form chords as free of beats as the most consonant chord in music, and yet more dissonant than the most extreme discord.[10]

2. That dissonance continues to be perceived as such when two tuning forks are placed, one at one ear another at the other,

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[7] Akustik und Musik, I, 1-19.

[8] Sensations of Tone, 182-228.

[9] Stumpf, Musik und Akustik, I, 1-19.

[10] Stumpf, op. cit., 7, 8.

and both sounded too weakly for beats to be produced. The same is true of the cases of double hearing, where one ear hears a tone slightly higher than the other. Beats are out of the question here, but the dissonance is sometimes most painful.

3. That the greater awareness of beats in the lower region of the scale should mean a change in consonance corresponding to pitch changes. Such is not the case.

4. That the consonance of an interval might be expected to vary according to the timbre of the instrument, since different timbres mean different overtone relations. But this again is demonstrably not the case.

5. That in experiments on mistuning intervals made by pure tones, it has been found that a variation of four vibrations could be detected. This could not conceivably be true if judgments of purity were made possible only by characteristic overtone effects.

To the proposition that consonance is determined by the similarity between the two clangs, due to their possession of identical overtones, the answer is made that this cannot apply to simultaneous clangs, because the only possible effect of two identical overtones in such a case is that of mutual reënforcement.[11, 12] When the same overtone occurs in two clangs, sounded simultaneously, what will appear to the ear is simply one relatively loud overtone in the total sound complex. The Helmholtz theory, accordingly, makes it necessary to limit the explanation of consonance to successive notes; in short, it involves a transfer of the meaning of consonance, making it identical with melodic relationship. Meyer, who has given more attention to melody than any other experimenter, makes a sharp distinction between melodic relationship, which has to do only with successive tones, and consonance, which has to do only with simultaneous tones.[13] He shows in detail, for example, how Wagner's interest in a variety of melodic relationships was often at variance with the usual requirements of consonance. Melody is clearly seen to be

[11] Stumpf, op. cit., 15-19.

[12] Krueger, Differenztöne und Konsonanz, Archiv für die Ges. Psych. I, 214-216.

[13] Contributions to a Psychological Theory of Music.

one kind of ultimate relation, consonance another, and the one is not to be explained by the other.

Thus neither the identity of overtones nor the roughness of beats between overtones prove to be adequate criteria of consonance, and with their failure the Helmholtz theory falls to the ground, leaving the problem to be solved by his critics.

LIPPS

Lipps, whom we may mention next after Helmholtz, sets out from a single principle.[14] Consonance, according to his theory, rests on the rhythmic accord of unconscious excitations. Stated more in detail, his explanation is as follows. Each single physical vibration causes a separate unconscious process which Lipps describes as micro-psychic. This micro-psychic process is the imaginary middle term between the physical stimulus and the conscious effect. There is a one to one correspondence between the physical stimuli and the micro-psychic processes, but any number of the latter may fuse into an uniform conscious experience. Given two tones of 200 and 100 vibrations respectively, we obtain two series of unconscious experiences, one of 200 separate excitations, the other of 100. Now since 2 : 1 is a simple rhythmic form, we have a micro-rhythmic experience of a simple and therefore pleasant sort, fusing into the conscious effect of the octave consonance. If micro-psychic processes are excited in such a way that every third member of one series corresponds with every second of the other, we get a less simple micro-rhythm and a less consonant effect in consciousness,—the fifth. Beyond a certain point of complexity the rhythm appears in consciousness as the characteristically unpleasant effect of dissonance.

It is obvious at once that the "psychic" effects with which Lipps deals are not given in any state of awareness, but must be deduced from the conscious effects they are intended to explain. This is an instance of the "explanation" of the conscious by the subconscious, an explanation which, as Professor Münsterberg has shown, amounts to nothing more than an acceptance of the problem itself as its own solution.[15, 16] Such a method of procedure is so far removed from laboratory experiment that it seems to mark a retrogression from Helmholtz back to the speculative vagueness of his predecessors, and for this reason it has met with rather caustic treatment at the hands of those for whom psychology ceases to be scientific when it ceases to be empirical. As psychologists we are permitted to describe only the facts; hence, when Lipps goes beyond the realm of psychic experience to one of unconscious, unverifiable facts, his position is interpreted as a capitulation on the part of psychology. Meyer, for example, speaks of the Lippsian law of tone relationship as "linguistically derived" from a "pseudo-experience of micro-rhythm."[17]

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[14] Grundtatsachen des Seelenlebens, Ch. XI.

More specific are the criticisms of Stumpf and Krueger, who meet Lipps on his own ground with a view to proving his deductions at fault. As against the Lippsian assumption that the tone sensation c′, corresponding to 256 vibrations, is due to the fusion of 256 separate unconscious waves, Krueger points out[18] that the stopping of a number of holes in a siren does not alter its characteristic quality of tone; in other words, that irregular interruptions of the physical succession of vibrations are without effect on the singleness of quality of the tone as consciously experienced. And even granting the reality of unconscious rhythm, it may be urged that the 5:6 ratio of the minor third and the 5:8 of the minor sixth make very good consonance, but very bad rhythm as we ordinarily think of the latter. More difficult still is the situation when we pass from intervals to chords.[19] The 4:5:6 ratio of the major triad, considered as rhythm, would be so confused that it might as well be 7:8:9. And there is the further objection, stressed by Stumpf[20] that

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[15] H. Münsterberg, Psychotherapy, Ch. VI.

[16] H. Münsterberg, in Subconscious Phenomena, I.

[17] M. Meyer, Unscientific Methods in Musical Esthetics, Journal of Philos., Psych. and Scientific Methods, 1904, I, 707-715.

[18] Kreuger, Differenztöne und Konsonanz I, 218, Archiv für die Ges. Psych. I.

[19] Krueger, op. cit., 220.

[20] Stumpf, Konsonanz und Dissonanz, 28 ff.

no matter how simple the tonal ratio, yet, owing to differences of phase, we hardly ever have physical coincidence of vibrations. These differences of phase are without effect on the consonance of the interval, but they would completely alter the character of the rhythm.

In order to meet such objections Lipps, after having assumed a whole realm of unconscious micro-rhythms, is obliged to assume further that there are possibilities in it which do not exist in the realm of conscious rhythm.[21] By the time micro-rhythm has been specially adjusted to meet all these requirements it remains with little more psychological significance than Euler's "unconscious recognition of number relations" proposed nearly two centuries ago.

KRUEGER

The theory of Krueger, which we may next consider, although in point of time it actually followed that of Stumpf, is a return to the more scientific method of Helmholtz, in that he attempts to refer consonance and dissonance to simple psychic facts verifiable by experiment.[22] But whereas Helmholtz emphasized largely the effects of overtones, Krueger attaches the greatest importance to difference tones.[23] He sets out with the general statement, deduced from his experiments, that the five difference tones of two simultaneous tones are to be calculated by always subtracting the two remaining smallest vibration numbers.[24] Thus for the ratio 1 : 2 the only possible difference tone would be 1, which would coincide with the lower generator; for the ratio 4 : 5 the difference tones would be 1, 3, 2, 1, 1, of which three fall together. An examination of each particular case of consonance and dissonance shows that not only do all the dissonant intervals contain difference tones which beat, but each one contains, as a result of the closeness of its difference tones, at least one mistuned unison,—that is, two tones lying so near together that they cannot be distinguished as two, but give an intermediate tone of a peculiar blurred character.[25] Consonances, on the other hand, are without exception free from difference tone beats, and contain only pure unisons, since the difference tones either fall exactly together or at least the distance of a third apart. The psychic effect of this physical difference is the clarity of the consonances as contrasted with the blurred and heterogeneous quality of the dissonances. A further significant physical difference is that the number of separate difference tones in the consonance is comparatively small, because of the five at least two fall together. On this account the consonances approximate the effect of unison more nearly than the dissonances.[26] Since the unison is the earliest and most familiar tonal experience of the ear, an approximation to this appears as the psychic effect of familiarity, an effect which is enhanced by the fact that the consonance itself is more frequently heard than the dissonance, and is therefore more familiar on its own account.[27] Taken together, all the characteristic relations of difference tones to consonant and dissonant intervals bring it about that the consonances are relatively clear, simple, and familiar; the dissonances are blurred, complex and strange.

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[21] Tonverwandtschaft und Tonverschmelzung, Zeitsch. für Psych. XIX, 1-40.

[22] Differenztöne und Konsonanz, Archiv für die Ges. Psych. I, 254 ff.

[23] Krueger, op. cit. 255 ff. and II, 1-80.

[24] Op. cit., I, 270.

This theory has found an uncompromising critic in Stumpf. The position taken by him in his first article, entitled "Differenz-Töne und Konsonanz,"[28] is one from which he has receded but little in all the controversy which has followed. [29, 30] He begins by raising the question of fact as to the relation of difference tones stated by Krueger. Passing that by, he argues that even if Krueger's findings be admitted, they still fail to provide an adequate criterion of consonance and dissonance as we distinguish them in musical experience. Krueger's physical test of a perfect consonance is, as we have seen, the absence of beats and of intermediate tones among its difference tones. But Stumpf[31] calls attention to the fact that the interval 8 : 11, an extreme dissonance, gives the difference tones 3, 5, 2, 1, 1. The smallest interval here is a fifth, hence there can be no difference tone beats or intermediate tones. It should therefore be a perfect consonance! Stumpf[32] cites a large number of more or less dissonant intervals, such as 11 : 15, 13 : 18, 12 : 17, 7 : 10, in all of which the smallest difference tone interval is a major third. All should, therefore be consonances, but not one of them is heard as such. He shows further the possibility of constructing triads which contain no difference tone beats, but yet are dissonant in every interval. Such, for example, 3 : 7 : 10; 7 : 10 : 17; 10 : 17 : 27. And, lastly, it is possible to rule out difference tones altogether by sounding two tuning forks very softly one in one ear, the other in the other, but this makes absolutely no difference as to the consonance of the interval.[33]

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[25] Op. cit., II, 15.

[26] Op. cit., II, 38.

[27] Op. cit., II, 42.

[28] Stumpf, Zeitschrift für Psych. 1905, Vol. 39, 269-283.

[29] Krueger, Psych. Studien, I, II, IV, V.

[30] Stumpf, Zeitschrift für Psych. Vols. 55, 59.

Not only does the elimination of the effects described by Krueger fail to alter the characteristic quality of a dissonance, but the artificial production of corresponding effects in a consonance does not alter its consonance. Stumpf points out that if we play the triad c-e-g, and play in addition to it low beating tones in the base, the triad keeps its characteristic quality.[34] The properties of the lower part of the tone complex do not go over into those of the upper part. Exactly the same is true of the colliding overtones on a piano, which do not make a given triad less consonant. In short, the criteria of consonance must be peculiar to the tones as such, and not due to any external feature superadded to them.

STUMPF

There remains Stumpf's own theory, which at present enjoys a wider recognition than any other. As against Lipps, he maintains that the criterion of consonance must be sought in conscious experience; as against Helmholtz and Krueger, that it must be a quality inherent in the tones themselves. After eliminating all other possible psychical conditions, he concludes that the ultimate psychological factor is to be found in the fusion of simultaneous tones; that is, in the relation of two sensory contents by which they form not a mere sum but a unit.[35, 36] The general law is, briefly, the greater the fusion the more perfect the consonance.

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[31] Stumpf, Differenz-Töne und Konsonanz, Zeitsch. f. Psych. 39, 273.

[32] Op. cit., 273.

[33] Op. cit., 274 ff.

[34] Op. cit., 278 ff.

Fusion, according to Stumpf, is something quite distinct from unanalysability. In fact, the fusion of a given interval is distinctly present as such to consciousness only after analysis has taken place. It is the qualitative unity that persists in a chord after its real unity, *i.e.,* its indistinguishability, has given way to an awareness of separate intervals.[37] This residual qualitative unity, says Stumpf, is an original relation, just as a sensation of color, only to be learned by examples, and not to be referred to anything more ultimate psychologically.

The validity of Stumpf's psychological position depends, as Krueger has shown,[38] in a large measure on his ability to maintain clearly the difficult distinction between the unity which disappears with analysis and the unity which remains after analysis. Here is a difficulty of which he has acquitted himself only imperfectly, and sometimes not at all. His experimental demonstration of the various fusion degrees is made according to difficulty of analysis.[39, 40] Stumpf justifies this by saying that, other things being equal, a low degree of fusion will actually mean greater ease of analysis. His tests were made with unmusical persons whose powers of analysis would naturally be comparatively low. He found that the likelihood of their judging an interval to be a single note was greatest in the case of an octave; next in order came the fifth, fourth, thirds, sixths and

[35] Tonpsychologie, II, 127 ff.

[36] Konsonanz und Dissonanz, Akustik und Musik, I, 34 ff.

[37] Konsonanz und Dissonanz, 43 ff.

[38] Archiv für die Ges. Psych. I, 236, Differenztöne und Konsonanz.

[39] Tonpsychologie, II, 142 ff.

[40] Konsonanz und Dissonanz, 36, 37.

sevenths.[41, 42] He argues that the particular difficulty of analysis experienced in these cases cannot be due to beats or overtones, since the results are the same when pure tones are used; it cannot be due to absolute distance, since the largest of the intervals, the octave, presents the greatest difficulty; it cannot be due to feeling value, since the greatest feeling accompanies hearing the thirds and sixths which lie in the middle of the series; and it cannot be due to practice, since the persons are unpracticed in music. Therefore, it must be the qualitative unity, or fusion, that makes distinguishability less in the case of the octave and more in the case of the seventh.

Manifestly Stumpf does not get beyond unanalysability as the experimental fact. Fusion, which is assumed to be distinct from difficulty of analysis, is after all arrived at only by a process of eliminating other possible factors in a given case of difficult analysis. The Stumpfian dilemma, as his critics see it, is that whenever he sharply separates fusion from difficulty of analysis, what remains is not a clearly understood psychological principle, but only a new name for the different degrees of harmony.[43] Nevertheless, in spite of considerable disagreement among various authors as to the exact meaning of tonal fusion, it is quite generally agreed that Stumpf has pointed the way to proper treatment of the problem.

It is not the purpose of this discussion to argue specifically for or against any of the consonance theories just mentioned. They all have their spokesmen who are contributing abundantly to the controversial literature of psychology. My own contention will be that all of them have, by giving too slight attention to the genetic aspect of the problem, left an inadequate account of certain facts which sooner or later must be explained. A complete theoretical account of consonance will have to reconcile the sharp sensory distinction between a consonant and a dissonant quality with the fact that the dividing line between consonance and dissonance has shifted in the last thousand years.

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[41] Tonpsychologie II, 145-179.

[42] Konsonanz und Dissonanz, 36, 37.

[43] Krueger, Op. cit., II, 235, 236.

If we examine Krueger's theory in this connection we see that he provides adequately for a fundamental breach between consonance and dissonance, but it is something given once for all. If all consonances are free from difference tone beats, while all dissonances contain them, we have indeed the basis for a sharp qualitative distinction, but it is one which would seem to preclude the possibility of change. Krueger does, to be sure, mention the fact that our more frequent hearing of an interval makes it more familiar,[44] but this is merely incidental to the fundamental criterion above mentioned.

Stumpf has had much to say about the genesis of music,[45, 46] but concerning the genesis of consonance his position is not unambiguous. In his *Tonpsychologie,* II, 208 ff., he takes up the question whether frequent combination is the cause of fusion. This question he answers in the negative as far as the possibility of psychic combination is concerned. But what is denied as a psychic possibility is, a few pages later, affirmed as a physiological possibility.[47] The physiological basis of fusion, he says,[48] is the specific synergy of the cortex, that is, "a determinate mode of coöperation of two nervous structures, having its ground in the structure of the brain, of such a kind that whenever the two structures give rise to their corresponding sensations there arises at the same time a determinate degree of fusion of these sensations." And on page 215 he makes the suggestion that "the mechanism of synergy, connate with the individual, may have been acquired in the course of generations, and perhaps the influences under which it took shape may have been in part mental influences. Impressions that affect the sense organ, together with relative frequency, might gradually set up a disposition to conscious fusion." He even goes further and ventures a few "vague conjectures," to use his language, as to how some of the early intervals were established. At this time he evidently admitted the probability that the fusion grades had been partly developed since the advent of the human race. He has, however, since modified this position, and in *Die Anfänge der Musik,* page 84, he makes the following statement: "In der Tonpsychologie legte ich die Meinung zugrunde, dasz in den Uranfängen des Menschengeschlechts die Verschmelzungsunterschiede doch noch nicht vollständig ausgebildet gewesen seien, was ich jetzt nicht mehr fur wahrscheinlich halte. Die Untersuchung der Sinnesempfindungen bei den heutigen Naturvölkern hat immer mehr gezeigt, dasz wesentliche Unterschiede gegenüber den unsrigen nicht vorhanden sind." And in the same book, page 27, he says of the original character of the fusion grades: "Diese Einheitlichkeit des Zusammenklanges ist . . . nicht eine Folge der musikalischen Entwickelung, sondern eine durch die Natur der Töne oder der ihnen zugrundeliegenden Gehirnprozesse notwendig bedingte Erscheinung." He proceeds here to suggest the probability that the octave is as consonant for animals as for us, the only difference being that they do not attend to its consonance relation as such. Two more statements will serve to make clear his present position as regards the evolution of consonance. In *Akustik und Musik,* Vol. I, p. 32, we read: "Es scheint eine Verschiebung des Lustgefühls von der vollkommensten Konsonanz gegen die unvollkommen hin stattzufinden, aber eine Verschiebung der Konsonanzverhältnisse selbst findet nicht statt." And, lastly, in his article entitled *Konsonanz und Konkordanz,*[49] "Konsonanz und Dissonanz sind nur graduell verschieden, und es kann der Grenzstrich an verschiedenen Punkten der Reihe gelegt werden, wie er denn tatsächlich von den alten schon nach der Quarte gezogen wurde, wahrend wir ihn nach der Terzen und Sextengruppen setzen und spätere Musiksysteme ihn vielleicht nach den Siebernen setzen werden." Taken together, the above statements indicate that the genetic problem of consonance that now exists for Stumpf is limited to the changing feeling attitudes toward the various intervals. The change of an interval from dissonance to consonance signifies for him nothing more than our growth in power to conceive an interval differently in a developing system of harmony. As to the actual degree of consonance or fusion of a given interval, that is something which has not changed since the beginning of the race. Such change as took place before then is to be accounted for in purely physiological terms; the influence of external stimuli is apparently of little account.

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[44] Op. cit., II, 42.

[45] Stumpf, Geschichte des Consonanzbegriffes; Abhandlungen der Bayerischen Akad. der Wissenschaften, Vol. XXI, 1901.

[46] Stumpf, Die Anfänge der Musik.

[47] Tonpsychologie, II, 211, ff.

[48] Ibid., 214.

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[49] Zeitschrift für Psychologie, Vol. 58, p. 341.

This negative attitude of Stumpf on the genesis of consonance seems to have been generally accepted without protest. The only article I have been able to find in which the opposite position is definitely stated is that of Professor R. M. Ogden.[50] He admits that Stumpf's arguments against the possibility of "inseparable associations"[51] are irrefutable, provided we regard the phenomena of consonance to be the result of an individual's tonal experience during his own lifetime, but thinks that "we have every reason for supposing that congenital associations result from racial experience of a definite and oft recurring sort, to produce instinctive modes of adjustment and attendant complexes of experience which are fundamentally unified." The last statement is in full agreement with the position to be defended here, but I disagree as to the irrefutability of Stumpf's arguments against the formation by the individual of "inseparable associations."

In the first place, a consonance, conceived as an association, does not possess the degree of inseparability which Stumpf assumed to be necessary in his *Tonpsychologie*. The interval c-a♭ is quite consonant so long as we imagine ourselves in the key of A flat. But imagine the same interval to be c-g♯ and it at once takes on a character of decided dissonance. Stumpf has given what seems to be the proper explanation of this fact;[52] to be sure, not the same explanation which he had given previously in *Akustik und Musik*,[53] but this point does not concern us here. The important fact is that if we conceive the imperfectly consonant minor sixth as an association, it is an association easily altered according to the musical context. In fact, musical disorientation easily affects our whole system of tonal relations if we are introduced for a time into a realm of amusical tone-steps. The usual acoustical perspective can by such a process be so altered that even the octave may appear equal to the higher fifth.[54]

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[50] R. M. Ogden, A Contribution to the Theory of Tonal Consonance, in Psych. Bull. VI, 297-303.

[51] Ibid., 299.

[52] Konsonanz und Konkordanz, 334, 335.

[53] Akustik und Musik, Vol. I, 104 ff.

But waiving the question as to how inseparable is the fusion of a consonant interval we may recall that certain observed facts, notably those of synaesthesia, favor strongly the view that relatively inseparable associations can, under certain conditions, be formed by an individual, especially during childhood. C. S. Myers[55] points out that in all his cases of colored audition it is the *individual* experience which seems to determine just what particular color will be associated with a given tone. A tone that is "seen" by one subject as green, appears to another as blue, and to another as yellow. "Two members of the same family, both endowed with synaesthesia, will hotly dispute the colors of certain words or vowels." Myers sees no possible explanation of these individual variations otherwise than as accidental personal associations. That this sort of association may be formed relatively late is indicated by the fact one subject reported on by Myers[56] acquired, by playing with figures at the age of twelve, the rather troublesome habit of seeing with every letter of the alphabet its individual number. This association, acquired at random, has persisted for eighteen years, although it is not invariably present. Myers thinks that the general setting in which such connections are found must be a strong tendency to a certain kind of association,—"a 'sympathy,' the psychological and physiological basis of which is unknown to us." It seems to me that in any consonance theory some such 'sympathy' as this must be assumed as the basis of the greater affinity of the simpler related tones, but this does not prejudice the possibility that the actual realization of definite fusion developments may take place during the lifetime of an individual. Certainly the cases of

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[54] L. E. Emerson, The Feeling Value of Unmusical Intervals, Harvard Psych. Studies, II, 269, 270.

[55] C. S. Myers, A. Case of Synaesthesia, Brit. Journ. Psych. 1911, IV, 228-238.

[56] Ibid., 238.

Myers leave little doubt that synaesthesia, at least, though in general a congenital endowment, may in its particular manifestations be the result of the individual's life experience. To be sure, the "associations" in these are sensory rather than ideational. But exactly the same is to be said of that peculiar association of two tones which causes them to be heard as relatively unified.

Stumpf argues[57] that if mere being in consciousness innumerable times necessarily involved fusion, the person who heard music frequently should be less capable of differentiating the two notes of an octave than one who heard music seldom. But "fusion," according to his own definition, is something distinct from analysability; it is a qualitative unity that persists unaltered even after the ear has discriminated the two elements involved. If Stumpf maintained his own definition with strict consistency he would have to admit that the very practice which improves the musician's ear in discriminating the two component tones of an interval may be at the same time fusing the same two tones into a more perfect qualitative unity. It is only his failure to make clear the psychological difference between the two kinds of unity—the numerical "Einsheit" which disappears with analysis, and the qualitative "Einheit" which remains after analysis—that enables him to argue, now as if fusion were identical with difficulty of analysis, now as if it were something altogether distinct.

The view to be maintained by the writer, that a tonal fusion means simply a highly permanent connection wrought under the direct influence of repeated sensory co-incidences, is rejected by Stumpf largely on "a priori" grounds; there is no record that this possibility was ever put by him to experimental test. The experiments to be reported in this paper were devised with the view to examining the characteristic changes effected by the frequent or prolonged hearing of certain intervals, it being my hope that an interpretation of these changes might throw light on the possibility of fusion changes denied by Stumpf. But before passing to the experimental results I shall state my own theoretical position more fully.

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[57] Tonpsychologie, II, 210.

CHAPTER III

A GENETIC THEORY

I shall begin with an hypothetical account of the genesis of consonance, the theoretical soundness of which will be tentatively assumed. After an examination of the historical and experimental facts to which it offers an explanation, I shall return to a discussion of theoretical points in detail. The tentative hypotheses with which we shall begin are:

(1) Consonance and dissonance have to do directly with the degree to which an individual is able to synthesize[A] two tonal elements into a unified complex; this synthesis is unaffected by analysis, and the individual's ability to analyse is unaffected by synthesis.

(2) Tonal synthesis may be conceived as an undertaking, attended by success or failure on the part of the hearing organism. Success is experienced as the characteristic quality of consonance, failure as that of dissonance.

(3) The inherent feeling value of a particular interval is a function of two factors—success and degree of difficulty. It is the barely successful synthesis, in which the individual accomplishes with difficulty the unification of a manifold, that he finds the keenest pleasure. If the synthesis is effected without effort he has little awareness of his accomplishment, and therefore little pleasure. If, on the other hand, in spite of his synthesizing activity, he is baffled by the complexity of the manifold to be unified, he experiences acute displeasure.

(4) A certain initial disposition to synthesis is pre-determined for a given interval by the simplicity of its numerical ratio. That is to say, we must pre-suppose a "sympathy" such as that which Myers postulates in the case of synaesthesia,—a more or

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[A] I shall use the expression "synthesis" instead of "fusion" in order to avoid the ambiguity which has come to attach to the latter term. By "synthesis" I mean simply a kind of fusion that is wholly independent of analysability.

less strong tendency to association, the strength of the tendency being proportionate to the simplicity of the ratio.

(5) The actual degree to which any given interval realizes its possibilities of synthesis depends upon the number of times the interval has been attentively heard by an individual or by his ancestors.[A]

(6) Between the distinct qualities of consonance and dissonance lies an intermediate zone of neutrality such as, for the present-day ear, 4 : 7 relation. In such cases neither success nor failure of synthesis is pronounced. It is characteristic of this zone that change due to frequency of hearing is more marked than for either the decided consonances or dissonances. Also we are likely to find the greatest individual differences in judgment of an interval when it has reached this stage.

Our task will be to show that the above assumptions, taken together, are the ones which best satisfy the facts. Now the facts with which we are concerned are of two kinds, those derived from the actual development of musical feeling, and those derived from experiments designed to test the conditions of consonance. We may examine the two kinds in order.

[A] The addition of the phrase "or by his ancestors" may seem to imply an anti-Weissman point of view on the question of acquired characteristics. I should be unwilling to admit, in the present state of that controversy, that such an implication would necessarily invalidate my position, but I do think it possible to account for the transmission, here assumed, as a purely social one. If thirds are acquired as a consonance by one generation they will be heard so much the more by the next, and will therefore become more completely consonant. We may accordingly interpret the degree of synthesis of a given interval either in Lamarckian fashion, as being in part a congenital association resulting from race experience, or, in accordance with Weissmannian principles, as resulting entirely from an individual experience which has had the benefit of the accumulations of the race. I am indebted to Professor B. M. Anderson for the suggestion that such accumulation of increments of advance as is here involved may be plausibly assumed in any society where the musical profession tends to run in families. The son, on account of his father's tutoring, reaches at an earlier age the point attained by his father, the grandson surpasses the son, and so on.

CHAPTER IV

EVIDENCE FROM THE HISTORY OF MUSIC

Historical evidence does not go back far enough to either prove or disprove the statement that the octave was originally a dissonance. To be sure, we have what at first might be taken as evidence to the contrary in the findings of von Hornbostel and others[58] to the effect that our present-day "primitive" peoples of Africa and Asia make abundant use of parallel octaves, and in a large number of cases, even of fifths and fourths. But it hardly needs to be argued that the conditions here are far from original. Even Stumpf, who makes much of anthropological evidence as proof that the present-day fusion grades were an original possession of the race, makes the following admission:[59] "Wir müssen freilich auch schlieszen, dasz das meiste, was bei den Naturvölkern an Musik beobachtet wird, keineswegs die allerprimitivsten Zustände darstellt, sondern mindestens in Hinsicht . . . des ganzen Aufbaues doch schon vielfach eine lange Geschichte hinter sich hat." And C. S. Myers,[60] speaking of the contamination of primitive music, expresses the opinion that unpolluted aboriginal music can hardly be found nowadays owing to the almost universal influence of the trader and the missionary. He says: "We have evidence of these conditions in the influence of Arabic and Portuguese tunes introduced into Africa, and in the spread of favorite native airs throughout North America and throughout Australia."

If we examine still more rudimentary conditions we again find that such evidence as is obtainable is inconclusive. Experiments have been made[61] to see whether a bird, by hearing its melody played after it in another key, could be induced to transpose. No variation on the part of the bird could be noticed. The

[58] E. M. von Hornbostel, Zeitschrift f. Angew. Psych. III, 465-487.

[59] Die Anfänge der Musik, 48, 49.

[60] The Ethnological Study of Music, Anthropological Essays.

[61] Anfänge der Musik, 11, 12.

nearest approach to transposition is a case reported by B. Hoffman.[62] He tells of a woodpecker who began by singing his song between c^3 and a^3, but with prolonged singing gradually flatted until he was singing the same song between a^2 f^3. However, as Stumpf remarks,[63] this seems to be due to a mere weakening of muscular contraction rather than to anything like a conscious transposition. There is so far not a shred of positive evidence that a bird has any conception of tone relation regardless of a particular pitch. Hence we may hardly suppose that an octave could be felt by it as in any relation to its fundamental. Such a lack, of course, does not mean that the two notes together would necessarily give a combinational quality of dissonance; we must rather suppose that at this stage everything except the particular note being sung is a meaningless confusion. But the later course of development will, I think, make plausible the assumption that the octave was the earliest dissonance. We may, at least, without prejudice to the facts that come later, accept this inferred fact as an historical starting point.

It is obvious that the octave enjoyed an early advantage over every other possible interval in its progress toward consonance. Its simplicity of ratio 1 : 2 predetermined for it a rank second only to that of the unison 1 : 1. And two circumstances led to a speedy development of its possibilities. For one thing, whenever any sound was heard that contained as many as sixteen overtones,[64] the interval of the octave would occur among the upper partials nine times, as compared with five fifths, four fourths, three major thirds, three major sixths, two minor thirds, two minor sixths, two natural sevenths, one major second, one minor second. For another, the natural difference of register for the voice of a man and a woman is such that in attempting to give the same tone they would naturally give two tones an octave apart.[65] In the primitive activity of calling signals, when every member of a company tried to hold fast to a certain tone, this must have happened often. The natural frequency with which the octave was heard would, according to the theory here maintained, bring it about that its degree of synthesis gradually increased until it passed the threshold of consonance. It was then felt as more pleasant than the unison because this success was as yet attended by considerable difficulty.

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[62] Anfänge der Musik, 78.

[63] Ibid., 78, 79.

[64] Stumpf, Tonpsychologie, II, 209.

[65] Ibid., 215, 216.

Now we actually find that among the ancient Greeks the octave had reached this point of pleasantness. They derived an acute pleasure from the practice of magadizing, that is, of having the men and boys sing in melody together in octaves. Thus in the Aristotelian Problems[66] we find the following question asked: "Why is antiphonal singing (i.e., in octaves) more beautiful than singing in unison?" This characteristic experience of the octave was, as we shall see, set apart as essentially different from any other possible combination of two notes, but it is a question how far the Greek separation of the octave from other intervals corresponded to our division of consonance and dissonance. Indeed the question of what kind of musical consciousness to attribute to the Greeks is one that has undergone an extraordinary change of interpretation within the last fifty years. Gevaert[67] is authority for the following statement: "Il fut un temps—qui n'est pas loin—où beaucoup de savants philologues et musiciens prétendaient dénier aux Grecs tout usage de l'harmonie simultanée. Quelques-uns allient plus loin, et voulaient même leur refuser la simple connaissance des accords de deux sons. LeMémoire de notre éminent helléniste Auguste Wagener sur la Symphonie des Anciens, publié en 1861, mit a néant ces opinions erronées." But if philologists and musical scholars before 1861 went too far in what they denied to the Greeks, those of to-day are surely swinging too far in the opposite direction. The commonly accepted view now is that the Greeks classed the octave, fifth and fourth together as consonances in exactly the same way that we do to-day. The only difference, it is maintained, between their consonance-dissonance division and ours is that for them the dissonance began with the thirds and sixths, whereas we include the latter among the consonances. Now if change has clearly taken place in the case of the thirds during the last 2000 years, and we shall see that there is no possibility of doubting this, there should be at least a certain presumption in favor of expecting development in the case of the fifths and fourths. Evidently the scholars since 1861 have not approached the Greek passages with this presumption in mind. It has lately been agreed to translate συμφωνία as "consonance"; and since the fifth and fourth are from very early times spoken of by the Greeks as συμφωνίαι, it is argued that they must have always been full-fledged consonances. But if one sets out by translating συμφωνία, not as consonance in a rigid sense, but as any interval easily available in music—including both perfect and imperfect consonances, and the milder dissonances as well—the whole literature takes on a new aspect. I think there is every warrant for this translation. Certainly the passages referred to by Stumpf in his *Geschichte des Consonanzbegriffes* bear readily the interpretation that the fifth was to the Greek approximately what the third was to the fourteenth century ear, that is to say, between the positions of the third and the minor seventh for the present-day ear. This intermediate value of the fifths and fourths would account for the comparatively late period at which a clear cut distinction between consonance and dissonance occurs. Neither in Aristotle nor in the Problems attributed to him is there any particular expression for dissonance.[68] But we do have abundant evidence that the octave was in almost every respect set apart from all other intervals. Thus we read in the *Problems* 35a,[69] Διὰ τί ἥ διὰ πασῶν καλλίστη συμφωνία; "Why is the octave the most beautiful συμφωνία?" Again, in Problem 18, Διὰ τί ἥ διὰ πασῶν συμφωνία ἄδεται μόνη; μαγαδίζουσι γαρ ταύτην, ἄλληνδε οὐδεμίαν. "Why is only the octave employed in singing? One magadises (i.e., sings in parallels) the octave, something which is done with no other συμφωνία." And in Problem 17: Διὰ τί διά πέντε καὶ διὰ τεττάρων οὐκ ἄδουσιν ἀντίφωνα; "Why are fifths and fourths not sung in parallels (i.e., after the fashion of the octave)?"

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[66] Gevaert et Vollgraff, Les Problèmes Musicaux d'Aristote, Problem 39a.

[67] Op. cit., p. 136.

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[68] Stumpf, Geschichte des Consonanzbegriffes, 22, 23.

[69] Gevaert et Vollgraff, op. cit., 19 ff.

And in Problem 39b there is a longer passage to the effect that the octave is the only *συμφωνία* which can be used at the beginning or end of a musical piece, since it alone procures for the mind the impression of equilibrium and stability.

These four passages leave little doubt that at the time of Aristotle, or shortly afterward, the fifth and fourth were markedly less consonant than the fifths of the ninth century or the thirds of to-day. They could not be sung in parallels, and they could not be used at the end of a piece. Moreover, they were less beautiful than the octave, whereas for us the most imperfect of the consonances is the most beautiful of them all.

There is, to be sure, some indication that parallel fifths and fourths were occasionally played on instruments. Plato (Laws, VII, p. 812) speaks of the use by the zitherist of antiphonic successions other than that of the octave. If Gevaert's[70] interpretation of this passage is right, it can only refer to the doubling of the melody in fourths and fifths. But it also seems not unlikely that players of instruments, so far as they did this, were taking a step in advance of popular practice. The following passage from the *Republic*[71] indicates a strong probability that at least some of the men who handled instruments were experiencing pleasure in effects which seemed absurd to the people at large. It reads: " 'Tis as good as a play to hear them talking of their condensed notes, as they call them; they put their ears alongside the strings like persons catching a sound from their neighbor's wall—one set of them declaring that they distinguish an intermediate note, the others insisting that the two sounds have passed into the same—either party setting their ears before their understanding." "These gentlemen who tease and torture the strings, and rack them upon the pegs of the instrument" would, according to the theory here defended, quite certainly have developed the fifth from its status as an intermediate interval to one of consonance. Their position, we may imagine, was not unlike that of the singers in the fourteenth century who were forbidden by the conservative Pope John XXII to sing thirds, on the ground that this interval served only to intoxicate and not to soothe the ear.[72]

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[70] Op. cit., 149.

[71] Jowett's Plato, Republic VII, 530 d, ff.

Those authors who hold that the fifths and fourths were consonances for the Greeks[73] begin by calling attention to the Pythagorean harmonic proportion 12:8:6, or c-g-c. But the inferences that can be made from this leave much to be desired. In the first place, it is not clear that the Pythagoreans had reference to the consonance of simultaneous tones. The term κρᾶσις employed by them in this connection, was very frequently used to refer to the combination of successive impressions, as of consonants and vowels in speech.[74] And further, it is not clear that their "consonances" were not predetermined to a certain extent rather by calculation and measurement than by the actual report of the senses. At least, according to Boethius,[75] the late Pythagoreans Nikomachus, Eubulides, and Hippasus, obtained each a different classification of the consonances because they founded their calculations on different arithmetical speculations! How remote the Pythagorean "harmony" may have been from the actual harmony of musical combinations, as experienced, may be judged by their much reiterated conception of the harmony of the spheres, which, if it means the simultaneous sounding of all the heavenly bodies, would make out of the cosmos the most atrocious discord.

Plato can be relied on even less than the Pythagoreans to give us an exact notion of the Greek consonances. According to him[76] the intimations of the senses are so imperfect that it is absurd to take pains in investigating the exact truth about them. Just as the true study of astronomy lets the heavens alone so the true study of music limits itself to the natural harmonies of mathematical relations, and reflects not on the harmony of sounds, but on that of numbers.

We have already seen that the only Aristotelean evidence concerning the consonance of the fourths and fifths is their inclusion among the *συμφωνίαι.* This word is defined by Aristotle as a numerical relation between a high and low tone. There is no mention of a specific difference between consonance and dissonance; in fact, no single word that we can translate by dissoance.[77] *συμφωνία* is usually referred to simply as λόγος or relation. Indeed, it is significant that Aristotle asserts, as against the Pythagoreans, Met. N. 6. p. 1093 a. 20-25, that there are more than three *συμφωνὶαι* ; Stumpf,[78] in order to translate the Greek by "consonance" in this passage, is obliged to assume that Aristotle refers here to the addition of such intervals as the double octave and twelfth. But if we accept the translation "musical interval," we may interpret this passage, more naturally, it seems to me, as the incipient recognition that smaller and more dissonant intervals than the fourth and fifth are beginning to impress themselves as musically useful.

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[72] Oxford History of Music, II, 91.

[73] Stumpf, Geschichte des Consonanzbegriffes, 6.

[74] Stumpf, Ibid., 8

[75] Stumpf, Ibid., 55.

[76] Jowett's Plato. Republic, VII, 530 ff.

By the time of Thrasyllos, first century B.C., the classification of intervals has become very definite,[79] but it is noteworthy that the fifth and fourth are put in a class distinct from the octave. Thrasyllos makes a threefold distinction, (1) antiphonies, including the octave and double octave; (2) paraphonies, including the fifth and fourth; (3) diaphonies, including the intervals mentioned in the other two classes. Five centuries after Thrasyllus, the same classification is used by Gaudentius, but a significant change has taken place. The fifth and fourth are ranked by him in the same class with the octave; the third is now a paraphony, and the diaphonies include the intervals more dissonant than the third and tritone.[80] If paraphony, as used by Thrasyllos, is at all comparable to the same expression as used by Gaudentius we cannot but conclude that as late as the first century, B.C., the fifths and fourths were still in the intermediate class, but that by the fourth century, A.D., they had developed into full-fledged consonances. And it is an interesting fact in this connection that whereas the Aristotelian Problems speak only of the octave as the most beautiful interval, Bacchius in the fourth century A.D. mentions both the fifth and the octave, with the fifth first in order.[81]

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[77] Stumpf, Geschichte des Consonanzbegriffes, 22.

[78] Ibid., p. 34.

[79] Ibid., 48 ff.

[80] Stumpf, Ibid., 69-71.

By the ninth century the fifth is obviously a new kind of harmonic experience. The Christian church has found that a more beautiful concord than the octave is to be obtained by having concurrent voices sing in fifths.[82] In its practice of "organizing," the "vox principalis" sang the melody and the "vox originalis" sang the accompaniment in the fifth below in parallel movement. The great pleasure which ninth century singers derived from this sort of music cannot be satisfactorily disposed of with the statement that they merely felt themselves to be diversifying the means by which a melody might be sung in unison. The writings of this period indicate clearly that the interval of the fifth made very much the same appeal to them that the third makes to us to-day. Thus Regino, in his *De harmonica Institutione,* written in 892,[83] "Consonantia est acuti soni gravisque mistura, suaviter uniformiterque auribus accidens. Et contra dissonantia est duorum sonorum sibimet permistorum ad aurem veniens aspera atque iniucunda percussio." Their distinction of consonance and dissonance was evidently quite as definite as ours; the only difference is that the third is as yet distinctly not included among the combinations described as "suaviter auribus accidens."

Taken as a whole, the evidence from the time of Aristotle to that of the ninth century points very strongly to a gradual advance of the fifth from being almost a consonance to being the most acceptable one. The reason why its development should follow next in order after the octave is obvious. Since it is the next simplest ratio 2 : 3, it requires further, according to our theory, only that it be impressed on the hearer sufficiently often. Now we have seen that as regards natural frequency of occurrence in overtones the fifth comes next in order to the octave, since it occurs five times in the first sixteen overtones of any

[81] Ibid., 68.

[82] H. E. Woolredge, Oxford History of Music, Vol. I, Ch. IV.

[83] Woolredge, op. cit., I, 46.

clang. Moreover, it was probably quite often produced in the earliest times by tenor and bass voices calling together naturally at medium pitch. And its development among primitive people would be peculiarly favored by their practice of singing simultaneous melodies. Of this characteristic primitive practice von Hornbostel observes:[84] "Alle diese Formen von Mehrstimmigkeit sind dadurch charakterisiert, dasz mehrere Melodien zwar gleichzeitig erklingen, aber doch noch als Melodien. Auf die einzelnen Zusammenklänge als solche wird wenig oder gar nicht geachtet." Now whenever in such a chaos of sound there appeared a fifth, with its initial advantage of simple ratio and frequent natural occurrence, it is obvious that it would tend to become fixed rather than any other dissonance, and each time it was thus attentively observed its proximity to consonance would be increased. This is exactly what seems to have happened in every case. Those of the primitive and semi-civilized people of to-day who have advanced beyond the stage of parallel octaves, have invariably acquired, as the next step, parallel fifths and fourths. This is true not only of their singing, but of their playing on instruments. Stumpf[85] observes that in China, Japan and Sumatra it is quite usual that instruments make fifths and fourths with each other or with the orchestra.

The shifting division between consonance and dissonance seems to have included the fourth among the consonances almost simultaneously with the fifth. Its development among the Greeks exactly parallels that of the fifth; all the primitive races to-day who treat the fifth as a consonance treat the fourth likewise; and the ninth century "organum" which in its simplest form gave only parallel fifths, gave also parallel fourths when the "vox principalis" was doubled an octave above, and the "vox originalis" an octave below. But the simultaneous development of these two intervals may be readily understood according to the principle of the equivalence of octaves. That is to say, the resemblance between any note and its octave is now so strong that the presentation of any given fundamental is practically equivalent to the presentation of its octave. Hence, whenever a fourth was sounded, it could be heard, not as a new interval different from the fifth, but merely as the old interval inverted—and even perhaps a little more interesting, because of the slight increment of difficulty. The same principle explains the later simultaneous development of thirds and sixths; it is indeed the fundamental fact in all the triad inversions of modern harmony.[86] It is significant, however, that by the time of Guido of Arezzo, the first half of the eleventh century, the foundation of the Diaphony or Organum was no longer the fifth, but the fourth.[87] The perfect fifth has ceased to be the barely consonant and therefore intrinsically interesting interval it had been two centuries previously. As it becomes more consonant it becomes less available as a parallel interval.

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[84] E. von Hornbostel, Ueber vergleichhende akustische und Musik-psychologische Untersuchungen, p. 484. Zeitschrift für Angew. Psych. III, 1909.

[85] Anfänge der Musik.

The major third 4 : 5 is the simplest of the dissonances that now remain, and history merely repeats itself when the third by frequent hearing gains a place among the consonances. The possibility of its attracting especial attention was obtained when the strict form of Diaphony, which was based entirely upon the traditional concords, was relaxed in favor of free Diaphony, which admitted sounds that were discordant, provided they were not introduced consecutively or on an accented beat.[88] We find it recognized in the eleventh century *Micrologus* of Guido of Arezzo, who illustrates the pleasant effect of coming to the unison by way of the major third.[89] This is at a time when the minor third is still forbidden even in passing. By the twelfth century there is evidence of the use of both major and minor thirds and sixths provided they are treated lightly. A twelfth century manuscript of the British Museum, published by Coussemaker in his *Histoire de l'Harmonie au Moyen Age*,[90] gives the following very explicit regulations with respect to the progression after thirds and sixths: "The minor third requires the unison after it; the major third, the fifth; the minor sixth, the fifth; the major sixth, the octave." These intervals at this time evidently had their "regular" resolutions in much the same sense that the dominant seventh has its regular resolution in the harmony of to-day. The interesting transformation which has taken place in the third in the last seven hundred years is described summarily by Parry,[91] when he says, "whereas in modern times a man feels that he wants something more when he is without it, in medieval times he wanted something more because he had it." A twelfth century writer, comparing his own sense of consonance with that of the Greek, might have made exactly the same statement in regard to the fifth; for this interval left the Greek with a sense of disturbed equilibrium,[92] whereas in the twelfth century it was the regular resolution of the major third and minor sixth. The exact steps by which the almost consonant third became generally recognized as a consonance cannot be traced with certainty. Riemann[93] thinks the practice of singing in parallel thirds and sixths appeared earlier in England than on the continent, and that it spread from England to France. This, however, is thought very doubtful by Woolredge.[94] What interests us especially here is the fact that it seems to have had for a considerable time the characteristic attribute of an intermediate interval; that is, it had become a consonance to some ears, while it was yet a dissonance to others.[I] Pope John XXII, in a decree —1322—forbidding the use of discant, said: "Yet for all this, it is not our intention to forbid occasionally, and especially upon feast days or in the solemn celebration of the mass . . . the use of . . . the octave, fifth and fourth which heighten the beauty of the melody. . . . Used in such sort, the consonances would both soothe the hearer and arouse his devotion."[95] This decree, which excluded thirds, was set aside just fifty-five years later, when official recognition was given to the Faulx Bourdon, a form in which consecutive thirds and sixths were the rule. Obviously the ultra-moderns in the musical taste of that time had continued to exert such a pressure that the conservatives finally came to acknowledge their point of view as the better one.

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[86] Stumpf, Konsonanz und Konkordanz, Zeitsch. für Psych. 58,334.

[87] Woolredge, op. cit., I, 63.

[88] Woolredge, op. cit., I, 66.

[89] Ibid., 66.

[90] See Woolredge, op. cit., Chap. VI.

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[91] Evolution of the Art of Music, 88.

[92] Aristotelian Problems, 39 b.

[93] Geschichte der Musiktheorie im IX-XIX Jahrhundert.

[94] Op. cit., I, 161 ff.

I. We have already compared this state of affairs with the one that probably existed in Greece at one time regarding the fifth. We shall see later that the natural seventh 4:7 to-day manifests the same characteristic.

[95] Op. cit., I, 91.

The decline of the fourth and fifth seem just about to have kept pace with the advance of the third. Thus Simon Tunstede, an English theorist of the year 1400, in his *Quattuor Principalia,* writes:[96] "Let him who is to discant avoid as much as possible the perfect concords (i.e., fifths and fourths), keeping his part in the imperfect intervals, that is to say, in the thirds, sixths and tenths above the tenor, and with these let him discourse, ascending and descending according as it may seem to him expedient and most agreeable to the hearer." Thus the fifth and fourth, as they grew more consonant, lost the attribute which invariably belongs to the least consonant of the consonances, namely, the pleasing effect as a parallel interval.

Two possible arguments suggest themselves for maintaining the essentially consonant character of the third despite the slowness with which its recognition developed. The first is the fact that the earliest third was not the natural major third 4:5 but the Pythagorean 64:81. And indeed Helmholtz[97] suggests this as a possible reason for its treatment as a dissonance. But he himself admits later on [98] that "even when the natural major third was included in the so-called syntono-diatonic mode of Didymus and Ptolemaeus, it was not recognized as a consonance." And besides, recent experiments have shown that an interval can be considerably mistuned while continuing to give its characteristic fusion effect. It would seem then that very little weight can be attached to the slight discrepancy of the Pythagorean ratio.

A second argument is that the medieval man was so bound by what had been established before him that he refused to admit anything for which he could find no precedent in tradition. Such

[96] Woolredge, op. cit., II, 111.

[97] Sensations of Tone, 190.

[98] Op. cit., 228.

an argument, if it did not concern the immediate facts of sense, would deserve careful consideration. Certainly the medievals had an extraordinary respect for established rules, but that they could have maintained in theory what their ears were constantly upsetting in practice, that they could have heard thirds as we hear them, and yet have described their impressions as they described them,—this passes understanding.

The primitive people of to-day among whom has been found the practice of singing or playing in passages of thirds are very rare, and even then it is always a question how far European influence has found its way among them. But at least it is certain that the third has invariably been the interval acquired next after the fifths and fourths. Thus G. Knosp[99] reports that the Annamites found the major triad beautiful, but the minor triad and all seventh chords were horrible and meaningless. Stumpf[100] reports that a member of the Siamese court orchestra, tested by him, thought our major triad pleasing, but disliked the minor triad and dominant seventh.

It might be argued that since the Siamese scale is totally different from ours, the liking of this subject for our major triad is an argument against gradually developing fusion grades. But such a conclusion does not follow from the facts. In the first place, it is to be noted that this Siamese, whom his fellows named as the best musician among them, had already gained some familiarity with European music both in Vienna and at home. He was accordingly so far spoiled in respect to being a typical example of Siamese musical evolution. And furthermore, it is certainly possible to find an interval acceptable without feeling it as a consonance. The twelfth century musician expressed satisfaction with the third—for him a dissonance—provided it was preceded and followed by a consonance, and we ourselves do as much for the dominant seventh which we still class as a dissonance. The distinction between degree of pleasure and degree of consonance is one on which every writer now insists.[101] But for this distinction we should have to consider the third at present more consonant than the fifth. And lastly, if the Siamese felt the various fusion grades in the same way as a European, it might be asked why he should have objected to passages of thirds,[102] and why he should have disapproved of the minor triad and of the seventh chord. So far as the facts presented warrant any judgment as to the Siamese's distinction of consonance and dissonance, we should have to say that for him the minor third is more dissonant than is the minor seventh for us, while the major third may or may not be.

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[99] E. von Hornbostel, op. cit., 486.

[100] Tonsystem und Musik der Siamesen, Akustik und Musik, III.

[101] Stumpf, Akustik und Musik, I, 30-34.

The difficulty of getting definite results from the questionnaire method with primitive people, or any people differing widely from ourselves, is illustrated by the attempts of von Hornbostel to get judgments from a group of North American Indians concerning various consonant and dissonant combinations.[103] Every possible answer was obtained with regard to every chord! Myers[104] in the article previously mentioned, refers with considerable dissatisfaction to the attempts so far made to investigate the affective value of various pairs and triads of tones presented to savages. He says: "It is clear that both native and European intervals should be presented, that the intervals should be sounded on native and not merely on European instruments, and that repeated judgments must be obtained before reliance can be placed on such comparisons. Investigators have hoped to arrive at an answer by playing primitive melodies on the piano, harmonizing them now in one way, now in the other. But the likes, dislikes, or indifferences of the natives, ascertained by such a rough method of experiment, cannot be accepted as trustworthy." The only test of this sort which I was able to make in connection with the present investigation was to try the comparative effect of parallel fifths and thirds on one Japanese and two Chinese students at Harvard. The answer from one of them was uniformly that they were "just the same." Both of the other men usually gave a similar answer, but so far as any preference was expressed it was for the fifths, despite the fact that they had lived in this country for a year and a half. The inconclusiveness of these results is typical of what seems likely to be obtained by any method short of a prolonged and systematic investigation. Surely, until an exact understanding can be guaranteed between the experimenter and his subject, the safest guide in the matter of consonance and dissonance is the actual musical practice of a people, and this we find in such cases is limited almost entirely to the intervals of the octave, fifth and fourth.

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[102] Stumpf, Tonsystem und Musik der Siamesen, 106.

[103] E. von Hornbostel, op. cit., 486.

[104] C. S. Myers, op. cit., 2.

If the test for the complete establishment of an interval as a consonance is its use in the final chord of a cadence,[105] the first historical example of the complete establishment of the third is in the Nesciens Mater, 1450,[106] which offers a cadence in which the middle voice passes to the third instead of the fifth. The minor chord was avoided in closes down to the time of Bach, and even Handel and Mozart occasionally indicate their desire to give greater finality to a minor piece by ending it with a major chord.[107] Once admitted as a consonance, the minor third has not relegated the major to a position of lower importance, but rather the constant interchanges of the two have tended to enhance the feeling value of both. It is practically impossible in our present diatonic system to get a succession of thirds which are entirely major or minor. What we get in any passage is a frequent alternation between the two; that is, between the slightly less and the slightly more consonant. This situation has been of immense advantage in maintaining the third as the most interesting interval. It has been thus doubly fortified against gradual reduction to the status of an "empty" interval as the octave and fifth before it.

Of the sixths little more need be said than that their development as consonances parallels that of the thirds. Although slightly more complex in ratio than the latter, they give, owing to the equivalence of octaves before mentioned, more the effect of inverted thirds than of intervals in their own right. Their

[105] Parry, Evolution of the Art of Music, 88.
[106] Oxford History of Music, II, 139-142.
[107] Helmholtz, op. cit., 217.

simultaneous development with the thirds is accordingly to be explained on exactly the same grounds as that of the fourths with the fifths,

The next simplest ratio is the natural seventh 4 : 7. Its effect is peculiarly interesting on account of the fact that its development has been arrested, owing to its exclusion from the diatonic scale. It remains to-day an intermediate interval, and we find, as is to be expected, wide individual differences of opinion as to its status. Stumpf[108] considers it intermediate between consonance and dissonance; Hohenemser[109] and the Lippsians in general consider it a pronounced dissonance; Helmholtz[110] thought it nearer to the consonances than the dissonances; and Krueger[111] and many others think it belongs distinctly among the consonances. These differences recall the state of affairs, already mentioned, in regard to the third in 1322, and make even more plausible the conjecture that has been made with regard to the fifth among the Greeks. They suggest further that if the 4 : 7 were included among the intervals in regular use, its frequent hearing would presently make it unquestionably consonant for all ears. This consonance would be less complete than its predecessors, but therefore all the more interesting. Unfortunately there is no instrument which makes possible with the 4 : 7 the kind of experiments which I am here reporting on the thirds, fifths, minor and major sevenths, but there is every reason to believe that when similar tests can be devised the results will be of the same kind.

By the time of Palestrina, 1524-1594, the division was definitely marked between the thirds on the one hand and the minor seventh 5 : 9 on the other. The change of attitude that has since taken place toward the latter interval is in every way indicative of a general progress in the direction of consonance. Palestrina treated the minor seventh just as the twelfth century musicians treated the third in free Diaphony. He allowed it, but only as an "unessential" discord; that is to say, as a suspension or passing note. It thus appeared in a transient juxtaposition of voices while the main interest was directed to the movement of the parts themselves. But, shortly afterwards, interest began to attach to the interval on its own account, and Monteverde, 1567-1643, upset all traditions by attacking the dominant seventh and leading tone seventh directly without preparation.[112] The more recent developments are familiar to every student of harmony. Not only was the dominant seventh later allowed to enter unprepared, but feeling gradually developed in favor of extending the same license to the secondary sevenths, until now any chord of the seventh may be directly introduced without giving offense. At the same time, the feeling that the seventh required a "regular" resolution has undergone a marked alteration. In many passages from Parsifal, for example, one can see that Wagner, by the same device of passing notes which Palestrina used to introduce a single interval of a seventh, contrives to produce transient artificial chords with sevenths which are succeeded by new chords with other sevenths. These passing note combinations mark the incipiency of parallel sevenths just as surely as the sevenths occurring frequently in half closes in *Tristan and Isolde* point to the ultimate possibility of a complete close containing a seventh. But the precaution which Wagner takes in introducing his sevenths only when attention is concentrated on the melodic progression of the parts, is brushed aside by his more modern successors. Strauss is to Wagner in this respect as Monteverde is to Palestrina. The consecutive sevenths which Wagner suggests more or less indirectly, are attacked by Strauss without any relieving contrapuntal device. The climactic parts of *Heldenleben,* and the succession of chords which comes at the end of the song *Morgen,* are but two of the numerous instances of consecutive dominant sevenths which before Strauss would have been considered absolutely impossible. More decided still is an example from Florent Schmitt's *Tragédie de Salome* in which we have a passage of ten successive sevenths.

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[108] Differenztöne und Konsonanz, Zeitschrift für Psych. 59, 164.
[109] Krueger, op. cit., II, 219.
[110] Sensations of Tone, 195.
[111] Krueger, op. cit., 219.

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[112] Helmholtz, op. cit.

It is needless to multiply instances of what contemporary orchestral compositions are coming more and more to abound in.

It might be argued that the composer is merely trying to add a novel interest to his work by daring to use progressions that would have been considered too bold by his predecessors. But a composer's choice of means is never entirely irrelevant to the potential capabilities of his hearers. His startling effects, if true products of art, must end by introducing them to a new and more ultimate pleasure. We must suppose that the great composer builds somewhat better than he knows, in that his radical harmony is not a mere bit of wanton freakishness, but a more or less unconscious finding his way to the next stage in the musical progress of the race. If this is true, the rapidly developing freedom of treatment of the minor seventh in present-day music urges strongly the experimental investigation of the effect of repeatedly hearing this dissonance which bids fair to become a consonance.

But, before proceeding to an examination of experimental results, it will be well to summarize the main points of historical development. The qualitative distinction between consonance and dissonance has at all times had most immediate reference to the self-sufficiency which always exists for the one and never for the other. The practical test of self-sufficiency, and therefore of consonance, we have taken to be the immediate effect of the interval in a passage of parallels. On the basis of this distinction we may say that the history of each of our present consonances has apparently been a gradual development as dissonance, followed by a relatively short transition period, and later a gradual development as consonance. The typical case, in which we may observe all the gradations, is that of the third. As a dissonance it was at first too harsh for any use at all, but afterwards was considered a useful interval if properly restricted. Then follows a transition during which some seem to have felt it as a consonance while others still considered it dissonant. Later comes its general admission as a parallel interval, and still later its use in the final chord of a cadence. Each stage, we have seen, had its characteristic pleasure value. As a decided dissonance it was essentially unpleasant; as a mild dissonance it was pleasant according to the context; as a bare consonance it was essentially pleasant, as for example to Monteverde, who confessed his inordinate fondness for thirds; when it becomes a more perfect consonance it begins to take on a more empty character, as is instanced by our present growing dissatisfaction with such music as that of Donizzetti. The history of the fifth exemplifies the end of this same general process; that of the minor seventh its beginning. The fifth, recognized by the Greeks as more like the octave than any of the other intervals, though most probably not at that time a consonance in our sense, appears in the ninth century as the sweetest of concord. A few centuries later it has lost in content until consecutive fifths are becomtaboo; and the present-day effect of the fifth reminds one of the answer of Rossini to the question, "What is more *langweilig* than a flute?" His answer was, "Two flutes." The fifth to-day is a most essential element in the structure of harmony, but considered *per se* it has an unpleasant barrenness for most ears, so that it is not used consecutively except for particular effects. The seventh, altogether too harsh for the twelfth century ear, comes later to give much pleasure in the mildly dissonant chord of the dominant seventh, and the boldness of contemporary radicals augurs for it an eventual transition to the recent status of the third, that of the most pleasant of consonances.

CHAPTER V

Experimental Evidence

The experiments which I shall report were performed on nine subjects during the winter of 1913-1914 under the direction of Professor Münsterberg and Doctor Langfeld in the Harvard Laboratory. The intention was to examine the characteristic effects of prolonging and repeating two consonances, the fifth and third, and two dissonances, the major and minor seventh, with a view to ascertaining whether the facts thus obtained would lend support to such an interpretation of the genesis of consonance as has been given above .

The requirements to be fulfilled by the instruments to be used were,

(1) That it be possible to sustain an interval for any length of time and at fairly uniform pressure.

(2) That two successive chords or intervals could be compared, each having been played for the same length of time and with the same amount of pressure.

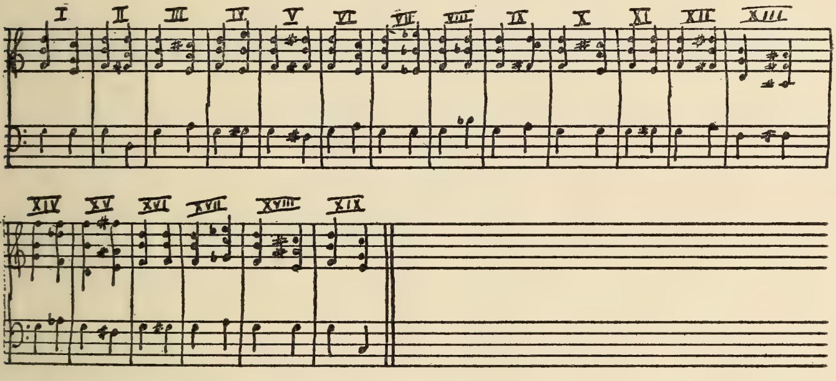
(3) That passages of any desired number of parallel intervals could be played at any speed desired.

The Tonmesser and harmonium are obviously not adapted for this last requirement; it was therefore necessary to use a cabinet organ with tempered scale. The one used was an instrument made by the Estey Organ Company, Boston. The stop exclusively used was a melodia with very prominent second, third, and fourth overtones.

PRELIMINARY EXPERIMENT

Since the final conclusions were to rest on an interpretation of the subjects' comparative judgments of intervals under different conditions, it was necessary first of all to compute for every subject a co-efficient of musical consistency obtained from a series of judgments under constant conditions. And, further, since the final results were to have to do with the question of consonance and dissonance, it was desirable to find out in advance how far the subject naturally inclined in his preferences toward the one or the other. In short, the intention of the preliminary experiment was twofold, to find out in the case of each subject his degree of consistency and his degree of consonance.

As stimuli for paired comparison in the preliminary experiment, I used nineteen resolutions of the dominant seventh chord. The expression "resolution" in these experiments is to be understood as meaning a pair of chords; the dominant seventh plus the chord into which it resolves. A comparison of resolutions hence means a comparison between two pairs of chords. The actual chords used, and their corresponding numbers as they appear in the tables follows. It will be seen that eight of the resolutions, 9, 10, 11, 12, 15, 16, 17, 18, are dissonant; the rest consonant.



Resolutions of Dominant Seventh Used in Preliminary Experiment.

Each of the nineteen resolutions was compared with each of the others in an irregular series, and then the same comparisons were made with the pairs presented in the reverse order. This made in all three hundred and forty-two judgments, given under conditions such that memory could not possibly have had any influence in determining the choices. The time during which each pair was sounded was kept constant by allowing two seconds for the dominant seventh and three for the following chord, making five seconds for the whole pair. An interval of two seconds was allowed between the resolutions.

In the individual tables, which follow immediately, the preferred resolutions are indicated by the numbers in the square. The two resolutions compared are to be found by passing from a given square upward to the number at the top of the column, and laterally to the number at the left side. Thus in the first table resolution number 1 was preferred to 2 both when it came before and when it came after it in order of presentation. The letters referring to the subjects throughout the tables are as follows:

B — Mr. J. W. Bridges
T — Mr. H. E. Burtt
C — Mr. R. A. Cutting
G — Mr. R. C. Givler
K — Mr. C. E. Kellogg
L — Dr. H. S. Langfeld
M — Miss O. Martin
S — Mr. W. M. Marston
R — Mr. A. A. Roback

TABLE I

B

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
2	1	×	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	=
3	1	2	×	3	3	3	7	8	9	3	3	3	13	3	3	3	3	3	19
4	1	2	3	×	4	6	4	8	4	4	11	4	13	4	4	4	4	4	19
5	1	2	3	4	×	6	7	5	5	5	5	5	13	5	5	5	5	5	19
6	1	2	3	6	6	×	7	8	6	6	6	6	6	6	6	6	6	6	6
7	1	2	3	7	5	6	×	8	7	7	=	7	13	7	7	7	7	7	19
8	1	2	3	8	8	6	8	×	8	8	8	8	8	8	8	8	8	8	19
9	1	2	3	4	5	6	7	8	×	9	=	12	13	14	9	16	9	9	19
10	1	2	3	4	5	6	7	8	10	×	=	12	13	14	15	10	10	10	19
11	1	2	3	11	11	6	11	11	11	11	×	11	13	11	11	11	11	11	19
12	1	2	3	4	5	6	7	8	9	10	11	×	13	14	=	16	12	12	19
13	1	13	13	13	13	13	13	13	13	13	13	13	×	13	15	13	13	13	=
14	1	2	3	4	5	6	7	8	14	=	11	12	13	×	15	16	14	14	19
15	1	2	3	4	5	6	7	8	9	10	11	12	13	14	×	15	15	18	19
16	1	2	3	4	5	5	7	16	9	16	11	12	13	16	15	×	17	18	19
17	1	2	3	4	5	6	7	8	9	10	11	12	13	14	17	16	×	18	19
18	1	2	3	4	5	6	7	8	9	10	11	12	13	18	15	16	17	×	19
19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	×

TABLE II

T

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	1	1	1	5	1	7	8	9	1	1	1	1	14	1	1	1	1	19
2	2	×	2	2	5	2	2	8	9	2	2	2	2	14	2	2	17	2	19
3	1	2	×	3	5	3	3	8	3	3	3	3	13	14	3	3	3	3	19
4	1	2	3	×	5	6	4	8	9	4	11	4	4	14	4	4	4	4	19
5	1	5	5	5	×	5	5	8	9	5	5	5	5	14	5	5	5	5	19
6	1	2	3	6	5	×	6	8	6	6	11	12	6	14	15	6	6	6	19
7	1	2	3	7	5	7	×	8	9	7	11	7	7	14	7	7	7	7	19
8	1	8	8	8	5	8	7	×	8	8	8	8	8	14	8	8	8	8	19
9	1	2	9	9	5	9	9	8	×	9	9	9	9	14	9	9	9	9	19
10	1	2	3	4	5	6	7	8	9	×	11	12	12	14	15	16	10	10	19
11	1	2	3	4	5	11	7	8	9	11	×	11	13	14	15	16	11	11	19
12	1	2	3	4	5	12	7	8	9	10	11	×	13	14	12	16	12	12	19
13	1	2	13	13	5	13	13	8	9	13	13	13	×	14	13	13	17	13	19
14	14	14	14	14	5	14	14	14	14	14	14	14	14	×	14	14	14	14	19
15	1	2	3	4	5	15	15	8	9	15	11	15	13	14	×	16	15	15	19
16	1	2	3	4	5	16	7	8	9	16	11	16	13	14	16	×	16	16	19
17	1	2	3	4	5	6	7	8	9	17	11	17	13	14	15	17	×	18	19
18	1	2	3	4	5	6	7	8	9	10	11	12	13	18	15	16	18	×	19
19	19	19	19	19	5	19	19	=	19	19	19	19	19	19	19	19	19	19	×

TABLE III

C

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	2	3	4	1	6	1	1	9	1	1	1	1	1	1	1	1	1	19
2	1	×	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	19
3	1	3	×	4	3	3	7	3	3	3	3	3	3	3	3	3	3	3	19
4	1	2	4	×	4	4	4	4	9	4	4	12	13	4	15	4	4	4	19
5	5	2	5	5	×	6	7	5	5	5	5	5	13	5	15	5	17	5	19
6	1	2	6	4	5	×	6	6	9	6	6	6	6	6	6	6	6	6	19
7	7	2	3	7	7	7	×	7	9	7	7	12	13	7	15	7	7	7	19
8	8	8	8	4	5	8	7	×	9	10	11	12	13	8	15	8	8	18	19
9	1	9	9	9	5	9	7	9	×	9	9	12	13	9	9	9	9	9	19
10	1	2	3	4	5	10	10	10	9	×	11	12	13	14	15	16	17	10	19
11	11	2	3	11	5	11	7	11	11	11	×	12	13	11	15	11	11	11	19
12	12	2	3	4	12	6	7	8	9	12	12	×	13	12	15	12	12	12	19
13	1	13	13	13	13	13	13	13	13	13	13	13	×	13	15	13	13	13	19
14	1	2	3	14	5	6	7	14	9	14	14	14	13	×	15	14	17	14	19
15	1	15	15	4	15	15	15	15	15	15	15	15	13	14	×	15	15	15	19
16	1	2	16	16	5	16	16	16	9	10	=	12	13	16	15	×	17	18	19
17	17	2	17	17	5	17	17	17	17	10	11	12	13	17	15	17	×	17	19
18	1	2	3	18	5	6	18	18	18	10	18	18	13	14	15	16	18	×	19
19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	×

TABLE IV

G

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	1
2	2	×	2	2	2	2	7	2	9	10	11	12	13	14	15	16	17	18	2
3	3	3	×	4	3	=	7	3	9	10	11	=	13	14	15	16	17	18	3
4	4	4	4	×	5	=	7	4	9	10	11	=	13	14	15	16	17	18	4
5	5	5	5	5	×	5	7	5	9	10	11	12	13	14	15	16	17	18	5
6	6	2	3	6	5	×	7	8	9	10	11	12	13	14	15	16	17	18	6
7	7	7	7	7	7	7	×	7	7	10	11	12	13	14	15	16	17	18	7
8	8	2	3	8	5	8	7	×	9	10	11	12	13	14	15	16	17	18	8
9	9	9	3	9	9	9	9	9	×	10	11	12	13	14	15	16	17	18	9
10	10	10	10	10	10	10	10	10	10	×	10	10	10	14	15	16	17	18	10
11	11	11	11	11	11	11	7	11	11	10	×	12	13	14	15	16	17	18	11
12	12	12	12	12	12	12	12	12	12	10	12	×	=	14	=	16	17	18	12
13	13	13	13	13	13	13	7	13	9	10	11	12	×	14	15	16	17	18	13
14	14	14	14	14	14	14	14	14	14	10	14	14	=	×	14	16	14	18	14
15	15	15	15	15	15	15	15	15	15	10	15	15	15	15	×	16	17	18	15
16	16	16	16	16	16	16	16	16	16	10	16	16	16	16	=	×	16	18	16
17	17	17	17	17	17	17	17	17	17	17	17	=	17	17	17	17	×	=	17
18	18	18	18	18	18	18	18	18	18	10	18	18	18	18	18	18	=	×	18
19	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	×

TABLE V

K

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	2	3	4	1	1	1	8	1	1	11	1	13	14	1	1	1	1	19
2	2	×	=	4	2	2	2	=	2	2	11	12	13	14	2	2	2	2	=
3	1	2	×	3	3	3	3	8	3	3	11	3	13	3	3	3	3	3	19
4	4	2	3	×	5	4	=	8	4	4	=	12	13	14	4	4	4	4	19
5	5	2	3	5	×	6	5	8	9	5	5	5	13	14	5	5	5	5	19
6	6	2	3	6	5	×	6	8	6	6	11	12	13	14	6	6	6	6	19
7	=	2	3	7	5	7	×	8	7	7	11	7	13	14	7	16	7	7	19
8	8	8	3	8	=	8	8	×	8	8	8	8	13	=	15	8	8	8	8
9	9	2	3	=	5	9	9	8	×	9	11	9	13	14	9	9	9	9	19
10	1	2	3	4	5	10	7	8	9	×	11	10	13	14	15	16	10	10	19
11	11	2	3	4	11	11	7	8	9	11	×	11	13	14	11	11	11	11	19
12	1	2	3	4	5	6	7	8	9	12	11	×	13	12	15	12	12	12	19
13	13	13	=	13	13	13	13	8	=	13	13	13	×	=	13	13	13	13	=
14	14	2	3	14	14	14	14	8	14	14	14	14	13	×	14	14	14	14	14
15	1	2	3	4	15	6	7	8	9	10	15	15	13	14	×	15	17	15	19
16	1	2	3	4	5	6	7	8	9	16	11	16	13	14	16	×	16	16	19
17	1	2	3	4	5	6	7	8	9	10	11	17	13	14	15	16	×	17	19
18	1	2	3	4	5	6	7	8	9	10	11	12	13	14	18	16	18	×	19
19	19	19	=	19	19	6	19	19	19	19	19	19	=	14	19	19	19	19	×

TABLE VI

L

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	1	1	1	5	6	1	1	1	10	11	1	13	1	1	1	1	1	1
2	1	×	2	2	5	6	7	2	9	10	2	12	13	2	2	16	2	18	2
3	1	3	×	4	3	3	7	3	9	3	3	3	13	3	3	3	3	3	3
4	4	4	3	×	4	4	4	4	4	4	4	4	13	4	15	4	4	18	4
5	1	5	5	5	×	5	5	5	9	5	5	12	13	5	5	5	5	18	5
6	1	6	6	4	6	×	7	6	6	10	11	12	6	14	15	6	17	18	6
7	7	7	7	7	5	7	×	7	7	10	11	7	7	7	15	7	7	18	7
8	8	8	3	4	5	6	7	×	9	10	11	12	13	8	15	16	17	18	8
9	9	9	9	9	9	9	7	9	×	10	11	12	9	9	9	9	9	9	9
10	1	10	3	10	10	10	7	10	10	×	11	10	13	10	15	16	10	18	10
11	1	2	3	4	11	11	7	11	11	10	×	11	13	11	15	16	17	18	11
12	1	12	12	4	5	12	12	12	9	10	12	×	12	12	12	16	12	18	12
13	1	13	13	13	13	13	13	13	13	13	13	13	×	13	13	16	13	18	13
14	1	2	3	4	5	6	7	14	14	10	11	12	14	×	15	14	17	18	14
15	1	15	15	15	15	15	15	15	15	15	15	15	15	15	×	15	15	18	15
16	1	16	3	4	5	6	7	16	9	16	11	12	13	14	15	×	17	18	16
17	1	17	17	4	17	17	17	17	17	10	11	17	13	17	15	17	×	18	19
18	18	18	3	18	18	18	18	18	18	18	18	18	13	18	18	18	18	×	18
19	19	2	3	4	5	6	7	8	9	10	11	12	13	14	15	19	19	18	×

TABLE VII

S

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	2	3	1	1	1	1	1	1	1	1	1	13	1	1	1	1	1	19
2	2	×	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	19
3	1	2	×	3	3	3	3	8	3	3	3	3	13	14	3	3	3	3	19
4	1	2	3	×	5	4	4	8	4	10	4	4	13	14	4	4	=	4	19
5	1	2	3	5	×	6	7	8	5	5	11	5	13	5	5	5	5	5	19
6	1	2	3	6	6	×	6	6	6	=	6	6	13	6	=	6	6	6	19
7	1	2	7	7	5	7	×	8	7	7	7	7	13	=	7	7	7	7	19
8	1	2	3	8	8	8	8	×	8	8	8	8	8	8	8	8	8	8	19
9	1	2	3	4	=	9	7	8	×	9	9	9	13	14	9	9	=	9	19
10	1	2	3	4	5	6	7	8	9	×	10	10	13	14	10	16	10	10	19
11	1	2	3	=	5	6	7	8	11	11	×	11	13	11	=	=	11	18	19
12	1	2	3	4	5	6	7	8	=	12	12	×	13	14	15	=	=	18	19
13	13	2	13	13	13	13	13	13	13	13	13	13	×	13	13	13	13	13	19
14	1	2	3	4	5	6	7	8	14	14	14	13	14	×	14	14	14	14	19
15	1	2	3	=	5	15	7	8	15	10	15	15	13	14	×	16	15	15	19
16	1	2	3	4	5	6	7	8	9	10	=	12	13	14	15	×	16	=	19
17	1	2	3	4	5	6	7	8	=	10	11	17	13	14	15	17	×	17	19
18	1	2	3	4	5	6	7	8	9	10	11	=	13	14	15	=	=	×	19
19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	×

TABLE VIII

M

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	2	3	1	1	1	1	1	1	1	1	1	13	1	1	1	1	1	19
2	2	×	3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	19
3	1	2	×	3	3	3	3	3	3	3	3	3	13	3	3	3	3	3	19
4	1	2	4	×	4	6	4	4	4	4	4	4	13	4	4	4	4	4	19
5	1	2	3	4	×	5	7	5	5	10	11	5	13	5	5	16	5	8	19
6	1	2	6	4	6	×	6	6	6	6	6	6	13	6	6	6	6	6	19
7	1	2	7	4	7	6	×	8	7	7	7	12	13	7	15	7	7	7	19
8	1	2	3	8	8	8	8	×	8	8	8	8	13	8	8	8	8	8	19
9	1	2	9	9	9	6	7	9	×	10	9	9	13	14	9	9	9	18	19
10	1	2	10	4	10	6	10	10	10	×	11	10	13	10	10	10	10	10	19
11	1	2	3	11	11	6	11	11	11	10	×	11	13	11	11	11	11	18	11
12	1	2	3	4	5	12	12	8	12	10	12	×	13	14	15	12	17	12	19
13	1	2	13	13	13	13	13	13	13	10	13	13	×	13	13	13	13	13	19
14	14	2	14	4	14	6	7	8	9	14	14	14	14	×	14	14	14	14	19
15	1	2	3	4	15	6	15	8	15	10	11	15	13	15	×	15	15	15	19
16	1	2	3	4	16	6	7	8	9	10	11	16	13	16	15	×	17	18	19
17	1	2	3	4	17	6	7	8	9	10	11	17	13	17	17	17	×	18	19
18	1	2	18	4	18	6	7	8	18	10	18	18	13	14	18	18	17	×	19
19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	×

TABLE IX

R

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	×	2	3	1	5	6	7	1	9	10	11	12	13	14	15	1	1	18	19
2	1	×	3	4	5	6	7	2	2	10	11	12	13	14	15	16	17	18	19
3	3	2	×	3	5	6	7	8	3	3	3	3	13	14	3	16	3	18	3
4	1	4	3	×	5	6	7	8	4	10	4	12	13	4	15	16	4	18	4
5	5	5	3	5	×	6	5	8	9	5	5	5	13	14	5	16	5	18	19
6	6	6	3	6	6	×	6	8	6	6	6	6	6	6	6	6	6	18	19
7	1	7	3	4	7	7	×	7	7	10	7	7	13	14	15	7	7	18	19
8	1	8	3	8	5	6	8	×	8	10	8	12	13	8	15	8	8	18	19
9	9	9	3	4	9	6	7	9	×	10	11	12	13	14	9	9	17	18	19
10	10	10	3	10	10	6	7	10	10	×	11	12	13	10	10	10	17	18	10
11	1	11	3	4	5	6	11	8	11	10	×	12	13	14	15	11	17	18	19
12	12	12	3	4	5	6	7	8	9	10	11	×	13	12	12	16	17	18	19
13	1	13	13	13	5	6	13	8	13	13	11	13	×	14	13	16	17	13	19
14	14	14	14	4	5	6	14	8	14	10	11	14	13	×	16	16	17	18	19
15	15	15	3	15	5	6	7	8	9	10	11	15	13	14	×	16	17	18	15
16	1	16	3	4	5	6	16	16	16	10	11	16	13	14	15	×	16	18	19
17	1	17	3	4	5	6	7	8	17	17	17	17	13	14	15	16	×	18	17
18	18	18	3	4	18	6	18	18	9	18	11	18	13	18	15	18	18	×	18
19	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	19	×

By representing the judgment "better than" by the symbol > we can express briefly each subject's order of preference according to the total number of votes for each resolution, as follows:

Bridges, $1>19>2>13>6>3>8>11=7>4=5>9>12>10>14>16>15>18>17$
Burtt, $19>14>5>8>9=2>1>3>13>7>4>11>16=15>12>17>6=10>18$
Cutting, $19>13>2>15>1>3>9>5>4=7>6>12>17>11>14>18>16>10>8$
Givler, $18>17>16>10>14>15>12>13>11>7>9>5>2=3>4>8>6>1>19$
Kellogg, $13>8>19>14>3>2>11>5>4>9>1>6=7>10>12=15=16>17>18$
Dr. Langfeld, $18>13>15>1>7=4>10=9=5=3>12>11>17>6>16=2>14>8>19$
Marston, $19>2>13>1>8>3>7>5>6>14>4>9>15>10>11>16>17>12>18$
Miss Martin, $19>2>1=13>3=4=6>8=10>7=11>14>9>18>15>5=17>12>16$
Roback, $6>18>13>3>5=10>7=8>14>15=16=4>17=11>19>1>12>9>2$

Now, if a subject is perfectly consistent in his judgments, we may expect that the preference of one resolution over another, as indicated by the total number of votes for each, will not be contradicted when the two resolutions are directly compared. Thus perfect consistency in the case of the first subject, Bridges, would mean that he will always prefer number 1 to any other resolution; 19 to any other resolution but 1; and 2 to any other but 1 and 19; also that 11, when directly compared with 7, ought to be judged equal to it, and so on. The method of computing the amount of deviation from perfect consistency may be seen by the calculation of Bridges' consistency for resolution number 4. This, according to the order of preferences by total votes, ought to have been judged <1, 19, 2, 13, 6, 3, 8, 11, 7; equal to 5; and >9, 12, 10, 14, 16, 15, 18, 7. It is compared directly with each of these twice; and whenever both of the direct judgments coincide with the order of preference we may grade the pair 1.00; if one agrees and the other disagrees, the grade will be .50; if one which should be judged equals gets one equals judgment, while the other is > or <, we may grade this pair .75. The coefficient for all the judgments with reference to a given resolution will then be an average of the percentages for the 18 pairs, and the final coefficient for the whole series of 342 judgments will be obtained by a general average of the 19 separate averages.

Bridges' results for resolution number 4 were:

Preference by votes.	By direct comparison.		Consistency.
4 < 1	<	<	1.00
19	<	<	1.00
2	<	<	1.00
13	<	<	1.00
6	<	<	1.00
3	<	<	1.00
8	<	<	1.00
11	<	<	1.00
7	<	<	.50
= 5	>	>	.50
> 9	>	>	1.00
12	>	>	1.00
10	>	>	1.00
14	>	>	1.00
16	>	>	1.00
15	>	>	1.00
18	>	>	1.00
17	>	>	1.00
		Average	.94

The above average represents the subject's consistency only for the number 4. This, when averaged with eighteen other averages, gives his general coefficient of consistency. The final coefficients, obtained in this manner, were as follows:

Givler	.92
Burt	.88
Marston	.88
Bridges	.87
Kellogg	.86
Miss Martin	.84
Dr. Langfeld	.78
Cutting	.76
Roback	.71

The classification of preferences according to their consonant and dissonant character shows an extreme preference for the dissonances on the part of Givler; Dr. Langfeld also shows a fairly distinct tendency of the same sort. The rest of the subjects prefer the consonances in varying degrees. The extremely "consonant" subjects are Marston, Miss Martin, Bridges, Kellogg; the other three—Burtt, Cutting and Roback—have the same general preference, but less marked. Ranking the subjects from the most "dissonant" to the most "consonant," we obtain the following order:

Givler
Dr. Langfeld
Roback
Cutting
Burtt
Kellogg
Bridges
Miss Martin
Marston.

FINAL EXPERIMENTS

The next problem was to find a method of recording any change in the degree of consonance of a given interval for the subject without suggesting to him the purpose of the experiment. Obviously little can be accomplished by merely playing a passage of intervals to him for five, ten or fifteen minutes, and then asking him to make a judgment whether the consonance of the interval has changed during that time. Not only is it impossible to eliminate a certain amount of suggestion in such a method, but it is hopeless to expect anything that approaches reliability in figures based on comparing pairs of impressions, one of which is pretty well obliterated at the time the judgment is made. We may find a clue to a better method by turning again for a moment to a consideration of the historical development of consonance. Musicians have always agreed[113] that the practical criterion of fully established consonance is the suitability of an interval in the final chord of a cadence. Now all the intervals that have so far met this requirement have fulfilled it only after having become the most acceptable intervals in parallel passages—parallel octaves among the Greeks, parallel fifths and fourths in the middle ages, parallel thirds in more modern times. The increase in acceptability as a parallel interval has been such an invariable feature of developing consonance that we may safely assume for our present purposes that the amount of decrease in dissonance may be measured in terms of this increase. But by the same token, further increase in fusion, once the interval has become consonant, will be marked by the decrease in its acceptability in consecutive parallels. That is to say, the interval which is just consonant is always at the height of its effectiveness for consecutive use. In becoming more consonant it becomes less available as a parallel interval.

We shall assume then as the measure of decreased dissonance the increase in the parallel value of an interval; and, as a measure of the increased fusion or synthesis of an interval already consonant, the decrease in parallel value. This is by no means an identification of consonance with the inherent pleasantness of an interval, for we have to do here not with isolated intervals, but with consecutive parallels.

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[113] Parry, op. cit., 88.

As before stated, the experiment is concerned directly with four intervals,—two consonances, the fifth and third; and two dissonances, the major and minor seventh. The problem is to find out whether the prolongation and repetition of these intervals produces anything which may be interpreted as a change in their degree of consonance; such a change, we have seen, can best be indicated by the amount of change in the acceptability of this interval as a parallel. How to determine the latter amount is our next question. The general method was that of paired comparison, with each judgment graded by the subject according to the degree of his preference. I began each hour of experiment by making out a table of graded comparisons of the four parallel intervals in question,—to which was added another, the minor ninth, in order to give a wider range of comparison. This table, which served as the standard for the day, was made up as follows: each interval, played consecutively in a passage of seven parallels, with c′ d′ e′ f′ e′ d′ c′ as the fundamentals, was compared with each other one played similarly. The subject was asked, in each case, not only to state his preference between the two passages, but also to grade the strength of his preference according to a scale A, B, C, D, E. A was to mean extremely strong preference, E extremely weak, while B, C, and D were intermediate grades. An example of the standard table follows:

Standard	3	5	7	VII	9
3	×	3C	3B	3B	3A
5	3C	×	5B	5B	5A
7	3B	5A	×	7C	7D
VII	3B	5B	7D	×	VIIE
9	3A	5A	7C	VIIE	×

Miss Martin,
Dec. 1, 1913

In all these tables, 3 means a passage of thirds; 5, one of fifths; 7, one of minor sevenths; VII, one of major sevenths; 9, one of minor ninths. The numbers to the left of the horizontal columns indicate the interval given first in presenting a pair for comparison; those at the top of the vertical columns indicate the second interval in the pair. Thus parallel thirds, coming first, were in this table preferred to fifths by C, to minor sevenths by B, to major sevenths and to minor ninths by A. Parallel fifths, coming first, were inferior to thirds by C, better than minor sevenths by B, better than major sevenths by B, and better than minor ninths by A.

The interval under consideration for this particular day was the minor seventh; the method was that of prolongation. After having recorded the standard table, I proceeded to play a passage of parallel sevenths, sustaining each interval exactly one minute. I did this for five minutes, playing thus five intervals of a minor seventh. Immediately after this I asked for new judgments of preference, in which the comparison was made only between the minor sevenths and the other parallels. The results of these judgments, at the end of five minutes, appear in the "first five minute" table below.

		3	5	7	VII	9
Standard	3	×	3C	3B	3B	3A
	5	3C	×	5B	5B	5A
	7	3B	5A	×	7C	7D
	VII	3B	5B	7D	×	VIIE
	9	3A	5A	7C	VIIE	×

TABLE TWO
(after five minutes)

	3	5	7	VII	9
3	×		3B		
5		×	5B		
7	3A	5C	×	7C	7B
VII			7D	×	×
9			7B		×

TABLE THREE

(after second five minutes)

	3	5	7	VII	9
3	×		3D		
5		×	7E		
7	3B	5C	×	7B	7A
VII			7B	×	×
9			7B		

When we compare the judgments of the sevenths in the standard table with those after five minutes of prolonging the interval, we find that it has lost one point to the thirds, has gained two from the fifth and three from the minor ninth. This gives it an increase in value of + 4 as compared with the standing of the seventh before prolongation. Repeating the same process for another five minutes, I obtained the results in Table Three. Here the minor seventh has gained two points on the third, six points on the fifth, three on the major seventh, and four on the minor ninth. Its value in this table is accordingly + 15 as compared with that of the standard table.

This process was repeated for a third and a fourth period of five minutes, sometimes for a fifth, but by this time the subjects expressed such general discomfort that it seemed hardly likely that the results could continue to have the same significance, especially as the hour was always nearly up at the end of the fourth trial. The use of a fifth period was therefore abandoned. The only other change in method which took place during the course of the experiments was in regard to the length of the prolongations. During the first few weeks of the experiments I used ten-minute periods, made up of five two-minute prolongations, and obtained the same type of results as later with the five-minute period, but the subjects indicated a far greater satisfaction in their attitude toward the five-minute length, and it was accordingly adhered to regularly after the first few weeks.

The method of repetition differed from that of prolongation in that each period, three minutes in this case, was occupied not with sustaining intervals, but with playing repeatedly an entire melody in parallel thirds, fifths, minor or major sevenths, as the case might be. The melody used was, with the exception of one note, within the compass of a sixth, so that the same notes recurred with great frequency. Three minutes sufficed for four repetitions of the entire melody. The first phrase of the melody, eight notes, was used in making the comparison for the tables. The amount of positive or negative change was determined, as before, by counting the difference between A and B one point, between A and C two points, and so on. The difference between an E preference in favor of an interval and one against it was also counted one point.[1]

The general tables which follow give for each of the four intervals the average amount of change from the standard at the end of each successive period. In the few cases where the periods were longer than five minutes, the result was recorded in the "second five-minute" table. Thus in the case of the first subject, Bridges, the only prolongations of minor sevenths were in ten-minute periods. The figures in each of the five-minute spaces represent an average of from one to four experiments.

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[1] The subjects were instructed to limit their introspection to the graded expressions of likes and dislikes, unless something of unusual interest occurred. The danger of accompanying paired comparisons by abundant general introspection is shown very clearly in the experiments of T. H. Pear on the major and minor triad.[114] Of his five subjects, the three who introspected least were by far the most self-consistent. His most introspective subject had a coefficient of consistency of 27.5 on a scale where zero was perfect consistency, whereas the coefficient of the least introspective subject was 3.64. It seems almost inevitable that the subject, in attempting to give an account of how he arrives at his judgment, leads himself to judgments that would normally not be made.

[114] British Journal of Psych., 1911, p. 57.

TABLE X

Method of Prolongation

Subject	Interval	First 5 Minutes	Second 5 Minutes	Third 5 Minutes	Fourth 5 Minutes
B	Thirds	−7	−2	−4	−10
Consistency .87	Fifths	−4	+4	+9	+6
Dissonance	Minor sevenths		+14		+8
Rank 7	Major sevenths	−1	+3	+2	+3
T	Thirds	+1	+.5	+.5	−2
Consistency .88	Fifths	−.5	−1.3	+1	0
Dissonance	Minor sevenths	+5	+5.7	+3	+5
Rank 5	Major sevenths	+3	+6.7	+5.5	+7.5
C	Thirds	+3	+2	+2	+3
Consistency .76	Fifths	+5	+3	−1	0
Dissonance	Minor sevenths		+3		+5
Rank 4	Major sevenths	+5	+1	+4	+6
G	Thirds	−11	−19.7	−15	−15
Consistency .92	Fifths	+2	+6	+4	+2.5
Dissonance	Minor sevenths	+4	+9.3	+7	+22.2
Rank 1	Major sevenths	+8	+15.5	+8	+19.8
K	Thirds	−4	−4.9	−3.2	−3.8
Consistency .86	Fifths	−2	−3.1	−1.3	−.5
Dissonance	Minor sevenths	−.8	−2.3	+1.5	+3
Rank 6	Major sevenths	−3.2	−1.5	−1.8	−3.2
L	Thirds	−4	−5	−6	−6
Consistency .78	Fifths	−4	−2	−4	−8
Dissonance	Minor sevenths		+4		+4
Rank 2	Major sevenths	+2	+3	0	0
M	Thirds	−5	−9	−9	−12
Consistency .84	Fifths	−1	−12	−5	+4
Dissonance	Minor sevenths	+4	+15	+10	+3
Rank 8	Major sevenths	+2	+4	+5	+3
S	Thirds	+.5	+.3	−1.5	−2.5
Consistency .88	Fifths	+1	+.5	+1.5	+1.5
Dissonance	Minor sevenths	+6	+2.3	+10.5	+7.5
Rank 9	Major sevenths	+7	+4.5	−1	+3.5
R	Thirds	−6	−9.3	−7	−5
Consistency .71	Fifths	+2	+3	−1	+2
Dissonance	Minor sevenths	+2.5	−1.5	0	+.5
Rank 3	Major sevenths	+4	+4	+7	+7

TABLE XI

Method of Repetition

Subject	Interval	First 3 Minutes	Second 3 Minutes	Third 3 Minutes	Fourth 3 Minutes	Fifth 3 Minutes
B Consistency .87 Dissonance Rank 7	Thirds	—3	—13	—3	—6	—6
	Fifths	—6	—9	—12.5	—9.5	—7
	Minor sevenths	+12	+10	+9	+10	+12
	Major sevenths	+4	+5	+5	+7	+4.5
T Consistency .88 Dissonance Rank 5	Thirds	0	+1	+2	+3	+4
	Fifths	+1	+1	0	0	—4
	Minor sevenths	+4	+4	+10	+10	+10
	Major sevenths	0	+5	+9	+10	+8
C Consistency .76 Dissonance Rank 4	Thirds	0	—3	—2	—2	—1
	Fifths	—6	—8	—5	—5	—3
	Minor sevenths	0	+3	+1	+7	+6
	Major sevenths	+2	+2	+1	+3	+7
G Consistency .92 Dissonance Rank 1	Thirds	—7	—9	—14	—14	—17
	Fifths	+1	+1	—5	0	
	Minor sevenths	+12	+6	+2	0	0
	Major sevenths	—2	0	0	+2	+5
K Consistency .86 Dissonance Rank 6	Thirds	+5	0	+1	—2	—1.5
	Fifths	+1.5	+1	+2	+.5	+1
	Minor sevenths	+2	—6	+5	+4	
	Major sevenths	—4.5	—5	—6	—6	—5
L Consistency .78 Dissonance Rank 2	Thirds	0	—1	—2	—2	—1
	Fifths	—2	+6	+1	+1	—1
	Minor sevenths	+10	+7	+5	+2	+5
	Major sevenths	+5	+6	+5	+3	+6
M Consistency .86 Dissonance Rank 8	Thirds	—16	—17	—18	—4	—14
	Fifths	—3	—2	—2	—1	—1
	Minor sevenths	+7	+13	+15	+12	+9
	Major sevenths	+5	+1	+1	+4	+13

TABLE XII

SUMMARY OF AVERAGES

Method of Prolongation

Interval	First 5 Min	M.V.	Second 5 Min.	M.V.	Third 5 Min.	M.V.	Fourth 5 Min.	M.V.
Thirds	— 2.9	3.6	— 6.2	5.3	— 4.0	3.9	— 6.6	4.4
Fifths	— .3	2.4	— .5	3.9	+ .4	4.2	+ .9	2.8
Minor sevenths	+ 3.0	1.9	+ 4.0	4.7	+ 5.0	3.8	+ 8.5	5.3
Major sevenths	+ 2.1	2.8	+ 4.3	2.9	+ 2.5	3.1	+ 7.5	5.0

TABLE XIII

Method of Repetition

Interval	First 3 Min.	M.V.	Second 3 Min.	M.V.	Third 3 Min.	M.V.	Fourth 3 Min.	M.V.	Fifth 3 Min.	M.V.
Thirds	— 3.0	5.7	— 6.0	6.0	— 5.1	6.2	— 3.9	3.7	— 5.2	6.1
Fifths	— 1.9	2.6	— 1.4	4.2	— 3.0	4.1	— 2.0	3.0	— 2.5	2.2
Minor sevenths	+ 6.7	4.0	+ 5.3	4.2	+ 6.7	3.9	+ 6.4	3.8	+ 6.8	3.4
Major sevenths	+ 1.4	3.2	+ 2.0	2.7	+ 2.1	3.6	+ 3.3	3.2	+ 5.3	3.5

The above results are represented graphically in the tables which follow. The last of these tables, number ~~XXII~~, is made from the statistics of C. W. Valentine[115] concerning the ontogenetic development of interval preferences. Reference is made to it later in the discussion.

XXVIII

INTERPRETATION OF RESULTS

It will be seen at a glance that whether we use the method of prolongation or of repetition, certain very characteristic tendencies appear for each of the four intervals. The third loses rapidly, the minor seventh gains equally rapidly, the fifth maintains a fairly constant level, the major seventh rises in value, but less rapidly than the minor seventh. It cannot be said that these changes are merely cases of a general adaptation which levels the distinction of pleasant and unpleasant. If this were true we should expect the greatest positive values in the case of the major seventh parallels, which are distinctly more disagreeable than those of the minor seventh. But the minor seventh actually has an average rise of 5.8 points as against 3.4 for the major seventh. Moreover, the lowest individual average value of the major sevenths,—3.1, was obtained in the case of a very "consonant" subject, Kellogg, who expressed the strongest distaste for them at the outset. And, further, the highest average positive value for the same interval, + 10.0, was obtained from the most "dissonant" subject, Givler, whose persistent bias in that direction we saw in the preliminary experiments on resolutions. Any argument to the effect that the processes which we have been describing merely tend to reduce affective differences is set at naught by the fact that there were actually 132 cases in which the minor seventh, after being judged inferior to the fifths or thirds in the standard table, was judged better in the prolongation or repetition tables. Thus Givler, after four five-minute prolongations of the minor seventh, preferred such parallels to any others; and remarked that to him they then seemed less like dissonances than like very full interesting consonances. His is, of course, an extreme case, but extreme only in exhibiting more of the same tendencies which others manifest to a less degree.

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[115] C. W. Valentine, Internal Preferences in Elementary School Children. Brit. Journ. of Psych., Oct. 1913, 190 ff.

Clearly the particular form of adaptation manifested here is most readily accounted for by the interpretation that a dissonance, when prolonged, becomes more suited for consecutive use; that is to say, more perfectly synthesized, in short, more nearly consonant. And since the region of highest affective value for an interval is the barely consonant region, we may expect the most rapid increase of value in that dissonance which is nearest this region; namely, the minor seventh. We actually find this to be the case almost without exception. We further expect to find the most rapid decline in the case of prolonged thirds, since they have reached their maximum value on becoming just consonant. This also is obviously indicated in the results. The fifth, being a more established consonance than the third, and therefore nearer its limit of synthesis, we should expect to show a lower initial value than the third, but a correspondingly greater stability in resisting the effects of repeated hearing. This is borne out by the results. When prolonged, the fifth changes almost not at all; when repeated, it declines, but much more slowly than the third. If this experiment had been made on a ninth century subject, we might have expected a very rapid decline of the fifth and rise of the third.

The fifths give in one or two cases certain curious results which call for especial consideration. The highest values regularly assigned to them in the standard tables were given by Givler and Kellogg, both violinists. Kellogg ranked them very nearly equal to the thirds at the outset, and gave a slight positive change after repetitions, a slight negative change after prolongations. There was nothing unusual in this except in the high initial value; but Givler always preferred the fifths to the thirds at the outset, and liked them still better after prolongation! His average prolongation value for fifths was + 3.4. When asked to introspect his attitude toward them, he stated that to him they seemed to have more masculine quality than the thirds; also that he felt less jar of movement in a passage of fifths than in one of thirds. Further, the fifth seemed so unified and self-sufficient that he did not think of it as empty.

This suggests very strongly the existence of a second stage in the development of an interval after its consonance has been well established, a stage in which it is apprehended by the hearer less as an interval and more as a reinforced unison. In this new capacity it is capable of taking on a new lease of interest. Further experiments will be necessary in order to determine the existence of such a secondary transition from empty interval to full unison. For the present the only evidence at hand is the comparison of the fifths with the octave. Statistics by C. W. Valentine[115a] on 62 men and 84 women indicate that in immediate pleasantness the octave is ranked by the men second among all musical intervals, preceded only by the major third; women rank it fourth, preceded only by the thirds and major sixth. In both cases it has a distinctly better value than the fifth, although it is distinctly the more consonant of the two. The stage of consonance which lies between the fifth and the octave thus appears to be marked at some point by an increase in affective

[115a] Op. cit.

value. A fairly adequate idea of the changes which take place in the affective value of the octave and fifth between childhood and maturity may be obtained by examining in Table XXVII the graphic representation of some of Valentine's statistics. He obtained two widely different groups of results, one from elementary school children, and the other from girls in a preparatory school. His object was to grade the intensity of likes and dislikes for all the musical intervals at the different ages between six and fourteen. The judgments given by his elementary school children are too chaotic to offer any sort of conclusive evidence. These children of the ages of six and seven "liked" all the intervals, "liked" them all very much the same, but the major seventh was preferred to the major sixth! Either the real facts were obscured by the inability of the subjects to conform to the simple requirements made of them, or else their likings were for mere noise as such. The second group, the girls in the preparatory school, were apparently in every way more alert than the first. Valentine says of them,[116] These children took a very keen interest in the tests, and most of them, even the very youngest, seemed very decided in their judgments." It sems therefore probable that the results given by these girls between the ages of nine and fourteen will represent to a certain degree the affective changes that take place during that period. His results for these years, combined with the statistics on adults, are represented graphically in the table. They show a general rise of the minor and major seventh, and a general decline of the major third from the age of nine to maturity. The fifths, with the exception of a marked fluctuation at the age of twelve, shows a slow and continuous increase up to maturity. The octave shows a marked rise between the ages of nine and thirteen, followed by a decline from then until maturity. These results offer in general a significant parallel to my prolongation tables, and in addition point to the existence of the particular kind of development in the fifth and octave which we have found suggested by my results in one subject. That is, the increase of the value of the octave in children, and of that of the fifth in my violinist subject, both alike indicate that when the unity of the developed consonance has reached a certain completeness, its effect may become enhanced by its increased dynamic value. The octave, which doubles the melody, and the fifths, used in organ mixtures, have acquired a new kind of interest in their capacity of re-enforcing the fundamentals. But let the octave or fifth be apprehended as an adhesion of two separate tones, as in the consecutive octaves and fifths of four part harmony, and the result is a most disagreeable barrenness.

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[116] Op. cit., 206.

CHAPTER VI

CONCLUSION

The facts obtained in the foregoing experiment, as well as those of musical history, favor the assumption that the degree of consonance increases as a result of the frequency with which an interval is heard. Moreover, each interval, in its development from dissonance to a high degree of consonance, undergoes a characteristic affective development corresponding to the different stages in its course. There is a continuous rise in affective value as we pass from the extreme to the slight dissonance; the bare consonance possesses the highest pleasantness of all intervals, but increasing consonance means for it a marked affective decline, until we reach the point where the interval gives the effect of a true unison, intensified dynamically; at this point follows a second affective rise, but by no means so great as that which marks the transition from dissonance to consonance. It goes without saying that the inherent affective quality of an interval may be greatly changed by the presence of such contributory factors as enter into any system of harmonic relations but this fact does not alter the fundamental fact that each consonance degree has its characteristic affective value.

The phenomenon of consonance, if the theory here advanced is right, is a special case of the adjustment of the inner to the outer relations. The nervous system, by a form of activity that tends with each repetition to become more simple and economical, gradually affects the synthesis of more complex physical relations. The affective aspect of the various stages of this process becomes more explicable when we consider that consciousness is most vividly present where new adjustments are being made. Where a synthesis has been accomplished so often as to be affected with ease, as in the case of the fifth, the output of consciousness is low as compared with that involved in synthesizing the third, where the accomplishment is relatively difficult. Now according to Professor Fite,[117] whose view I accept, the whole question of pleasure-pain is to be referred to the same conflicts which occasion consciousness. To quote from him: "Not only is conflict a condition of consciousness, but it is especially a condition of pleasure-pain. . . . Pleasure is succeeding, pain is failing in the process of resolving a conflict."[118] Using as an illustration a game of tennis, or of chess, he says: "All the feeling lies between two points—between that at which your opponent becomes strong enough to offer appreciable resistance and that at which he becomes so strong that any effort on your part is futile. If your opponent is too weak, struggle is unnecessary; if he is too strong, struggle is useless."[119] In another passage he says to the same effect,—"There is certainly a more intense pleasure in removing a stubborn obstacle than a pliant one. Pleasure depends not merely upon the prosperity of our undertakings, but upon the size of them, as estimated by the difficulties presented."[120] This last sentence presents, I think, exactly the point of view which we must carry to the consideration of the varying pleasurability of various intervals. It is thus that we can understand why different periods of history, different peoples of the same period of history, and the same individual at different periods of his own development can give such different accounts of the same interval. We may say that the fifth was the most pleasurable of all intervals for the ninth century hearer because, in the conflict which his synthesizing mind waged against the complexity of physical relations, he was just able to succeed with the 2:3 ratio. The third, 4:5, was slightly too difficult, but not so much as to discourage further hearing, especially if it could be immediately followed by a fifth. With a further advance, due to repeated hearing, the fifth becomes a relatively uninteresting affair, whereas the third, which can just be unified, acquires a tremendous vogue. Hence the amazing fondness for thirds at the time of Monteverde, hence the high value of the third for the young child of to-day, hence the rapid decline of the third when heard with great frequency by the adult to-day. The minor seventh, we may suppose, was almost discouragingly complex for the hearer in the tenth century,—as for Valentine's nine-year-old girls who gave it a value of — 44 as compared with + 52 for the major third. But with more and more frequent hearing it has been attacked with nearer success. Its rapid gain in my experiments is most clearly understood if we consider the 5 : 9 ratio as a tonal adjustment not quite completely successful. That it will later rank among the consonances we can scarcely doubt, if past successes are any index to the future. When it does become so, it will be the most pleasurable of all intervals, since its unification will make more demands on the mind than that of any of the other consonances. Exactly the same is to be said for the major seventh. At first it is disheartening in the opposition it offers to synthesizing activity, but the change that takes place with repeated hearing is, as a rule, definitely positive. Certainly it will take thousands, perhaps hundreds of thousands, of hearings to bring it about that its impression will be distinctly less dissonant than at present, but this is no reason for assuming that it will mark any exception to the law of consonance evolution.

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[117] W. Fite, The Place of Pleasure and Pain in the Functional Psychology. Psych. Rev. X, 1903, 633-644.

[118] Op. cit., 638.

[119] Op. cit., 637.

[120] Op. cit., 638.

What limits may we expect to the type of development here set forth? This is in effect the question asked in the introduction of the discussion. An answer is suggested by the following passage from Professor Fite: "Where an equilibrium has been reached it is certain to be disturbed by the coming to consciousness of further capacities which call for further realization. . . . The conflict is brought about by the increasing demands of the life purpose as opposed to the conditions that stand in the way of its realization, and the resolution of the conflict means that the obstructions are either removed or discovered to be definitely insurmountable."[121] Applying the same principle to that form of equilibrium which is experienced as consonance, we may say that there is no reason to expect that any one consonance-dissonance division will ever be ultimate. As soon as our tonal consciousness succeeds in the mastery of certain ratios, it tends necessarily to pass on to those that are a little more complex. The reduction of old consonances to emptiness, the establishment of new dissonances as consonances, this seems to be the typical form of a development to which it is impossible to set any limit. To be sure, there are certain tonal combinations which, to our present consciousness, mean only insurmountable difficulty. For example, if all the notes on a piano be struck at once, the effect is not even one of unpleasantness; it is musically insignificant. The case is comparable to that described by Professor Fite in which the resistance to be overcome is so strong that any effort is futile. This is an altogether different kind of experience from that of the harsh dissonances, such as the major seventh, for in experiencing the latter the hearer pits himself against them; his mind tries to synthesize them; he fails, and experiences excruciating displeasure. But as these extremely harsh intervals become gradually more acceptable, we must suppose that more and more of what now passes for chaotic jumble will become significant,—always painfully significant at first; and the direction of this progress will be from the more simple to the more complex mathematical ratios.

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[121] Op. cit., 640.

There remains the question whether the practical limits set by a particular system of harmony may not cut short an otherwise limitless progress. Stumpf touches on this question, in his article entitled "Konsonanz und Konkordanz."[122] He maintains here, as elsewhere, that the difference between a consonant and dissonant interval is a difference only of degree, and he admits, on different grounds from those which I have set forth, the possibility of an indefinite increase in the number of intervals that will be included among the consonances.[123] He accordingly atempts to explain the specific opposition of consonant and dissonant combinations, not as belonging to the common fundamentals of music, but only to the developed products of the harmonic epoch. The distinction of consonance and dissonance, which he limits in application to intervals, is gradual and susceptible to continuous change; but the distinction of concord and discord, which he applies to chords formed according to a particular structure principle, is one that will continue unaffected by the gradually enlarged scope of consonant intervals. Thus,—"Konsonanz und Dissonanz sind nur graduell verschieden, und es kann der Grenzstrich an verschiedenen Punkten der Riehe gelegt werden. Dagegen Konkordanz und Diskondanz sind spezifisch verschieden, und es giebt keinen Ubergang und keine Zwischenstufe, sondern nur ein Entweder-Oder."[124]

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[122] Zeitschrift für Psychologie, Vol. 58.

[123] Op. cit., 341.

The structure principle according to which our present system of concord has developed is, says Stumpf, that of selecting as a fundamental chord basis the greatest possible number of consonant intervals within the octave, taking them progressively according to their grades of consonance, from the stronger to the weaker degrees. Thus in the octave c-c′ we proceed from c first to the most consonant interval, the fifth, g; then to the third, e or e♭. We thus obtain as fundamental elements the triads c-e-g, and c-e♭-g. By repeating the same process with the two notes most consonant with c, namely g and f, we obtain the additional triads, g-b-d, and f-a-c. With these we have the material for the whole of our present scale. Secondary triads are now formed on every note on the scale, and thus furnish the chromatic elements with which the concordant triads can be rationally altered. A concord is a pure triad form; that is, a tonal construction according to the principle of the maximal number of tones within an octave that can be consonant with the fundamental and with each other, when the consonances are arranged in order from the stronger to the weaker. A discord is a rationally altered concord; that is, a tonal construction in which a triad, by the substitution or addition of chromatic elements, thus progresses more necessarily and fluently to another related triad.

The above scheme, which is worked out with great consistency by its author, offers a solution to many theoretical difficulties, especially to the question why a consonant interval may sound either concordant or discordant according to its musical context. It is, in fact, the only attempt up to the present which adequately explains the extent to which our pleasure and displeasure can emancipate themselves from the merely sensory factors in musical combinations. But is not clear that even this well grounded opposition of concord and discord is one whose foundation elements will not undergo a further evolution. It is arbitrary to assume that the maximal number of consonances available for the construction of concords will always be the particular number at present in use. Stumpf is at considerable pains to justify his exclusion of the natural seventh, 4 : 7, from his original interval elements. He gives two arguments for building his system on the triad 4 : 5 : 6, rather than on a chord of four notes, 4 : 5 : 6 : 7. First, the chord 4 : 5 : 6 : 7, when played with the third a minor, would give a noticeable dissonance, 20 : 24 : 30 : 35, since the fifth 24 : 35 varies too widely from the pure fifth 24 : 36. Second, the modern scale, formed from triads of the more perfect consonances, has already established as its seventh the dissonant 5 : 9. The intrusion of the 4 : 7 would only cause disagreeable confusion of the 5 : 9 and 4 : 7; and, since the 5 : 9 already has the right of way, the 4 : 7 is rejected altogether.

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[124] Op. cit., 341.

But what, we may ask, is to be expected when the 5 : 9 also becomes a consonance? The argument from the necessary dualism of major and minor will no longer hold good, since with the 5 : 9 as seventh the minor chord c-e♭-g-b♭ offers even less difficulty than the major. In fact, the same chord, a-c-e-g, is treated almost as a concord by Leoncavallo in "Pagliacci." Likewise the argument from the confusion of sevenths will cease to obtain, since the new consonance in question will be an element already contained in the scale. If our system of harmony is to continue to be as rational as Stumpf makes it out to be at present, then the advent of the 5 : 9 as a universally accepted consonance will necessitate the reconstruction of the whole system of harmony on the basis of a chord of four instead of the triad 4 : 5 : 6.

We must conclude, then, that the present opposition of concord and discord is alterable for the same reasons as those that obtain for the opposition of consonance and dissonance. The only difference is that the latter rests on isolated sensory experience whereas the former is an antagonism which has been organized into a complete system. It will, therefore, offer a tremendous resistance to anything that makes for upheaval. But, if the point of view that has been maintained here is a sound one, we must expect that our whole system of harmony will gradually come to reflect whatever changes occur in the consonance degrees of its constituent intervals; and this latter type of change, we have seen, is one to which no limit can be set.

TABLE XIV

SUBJECT B
CONSISTENCY .87
DISSONANCE RANK 7

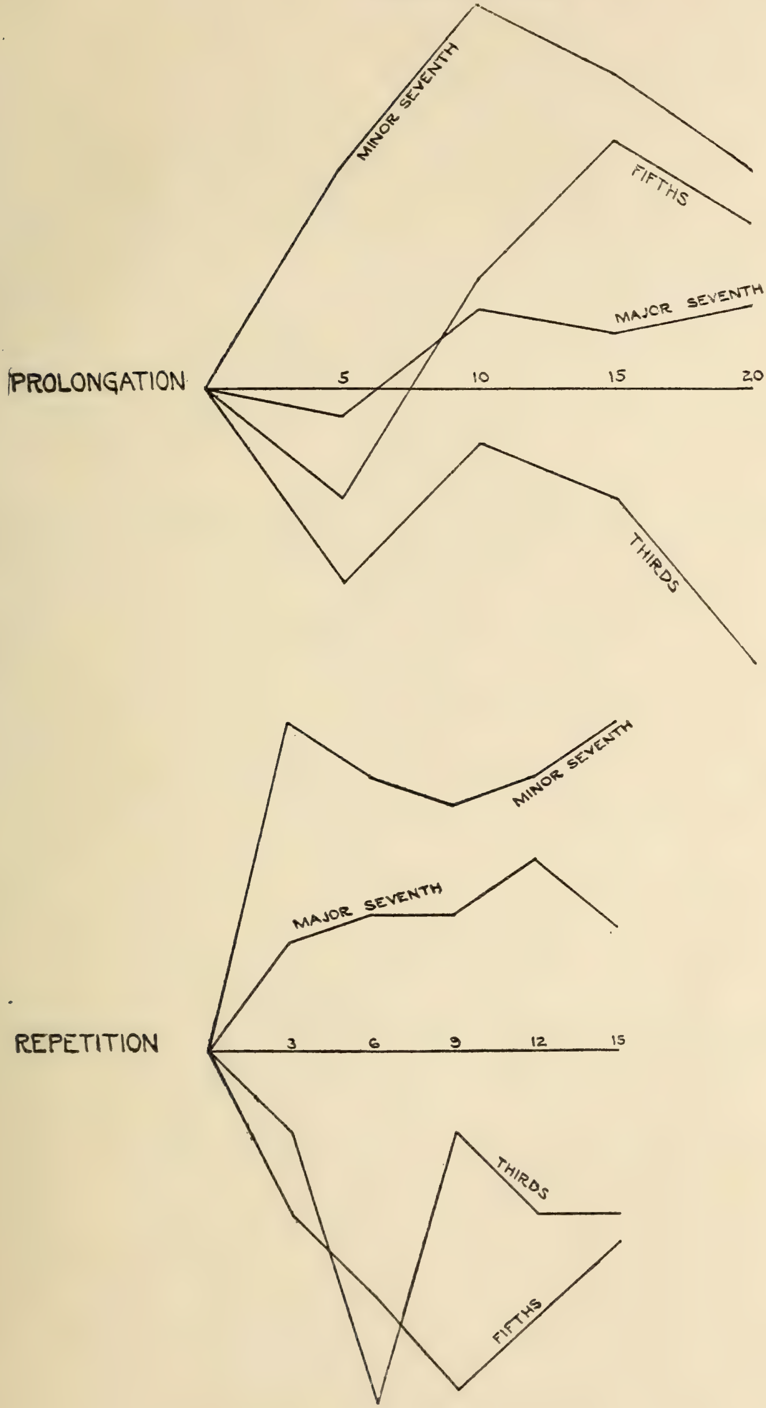
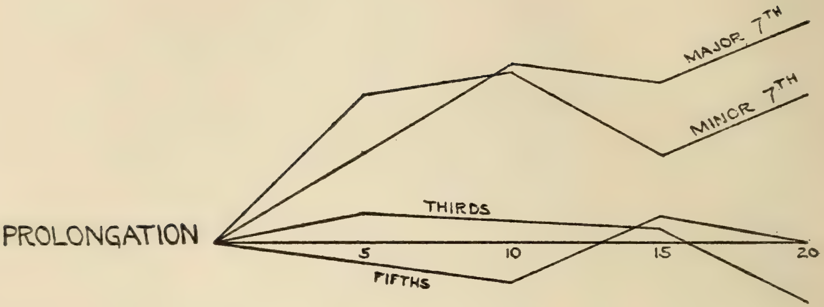


TABLE XV
SUBJECT T
CONSISTENCY .88
DISSONANCE RANK 5



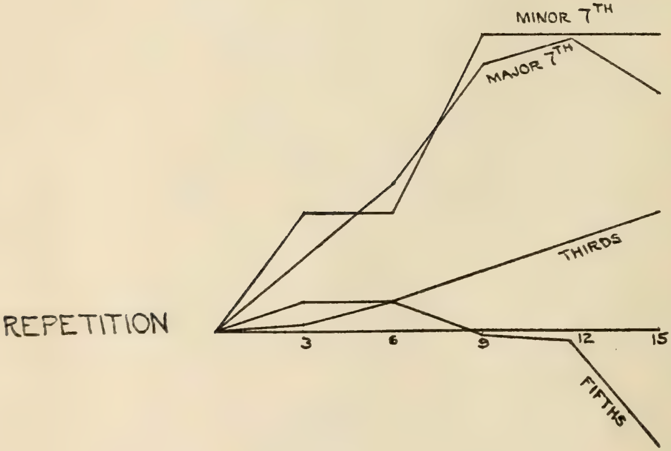
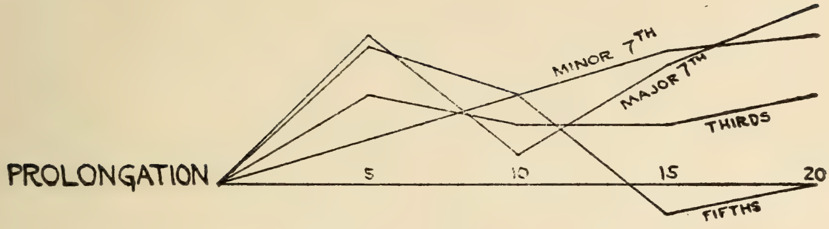


TABLE XVI

SUBJECT C
CONSISTENCY .76
DISSONANCE RANK 4



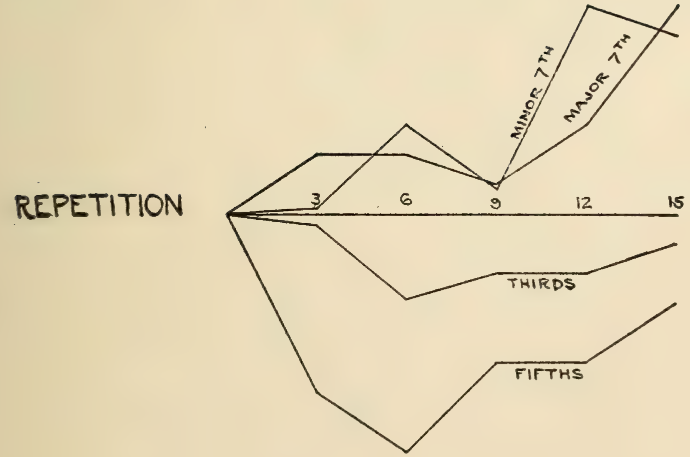


TABLE XVII
SUBJECT G
CONSISTENCY .92
DISSONANCE RANK 1

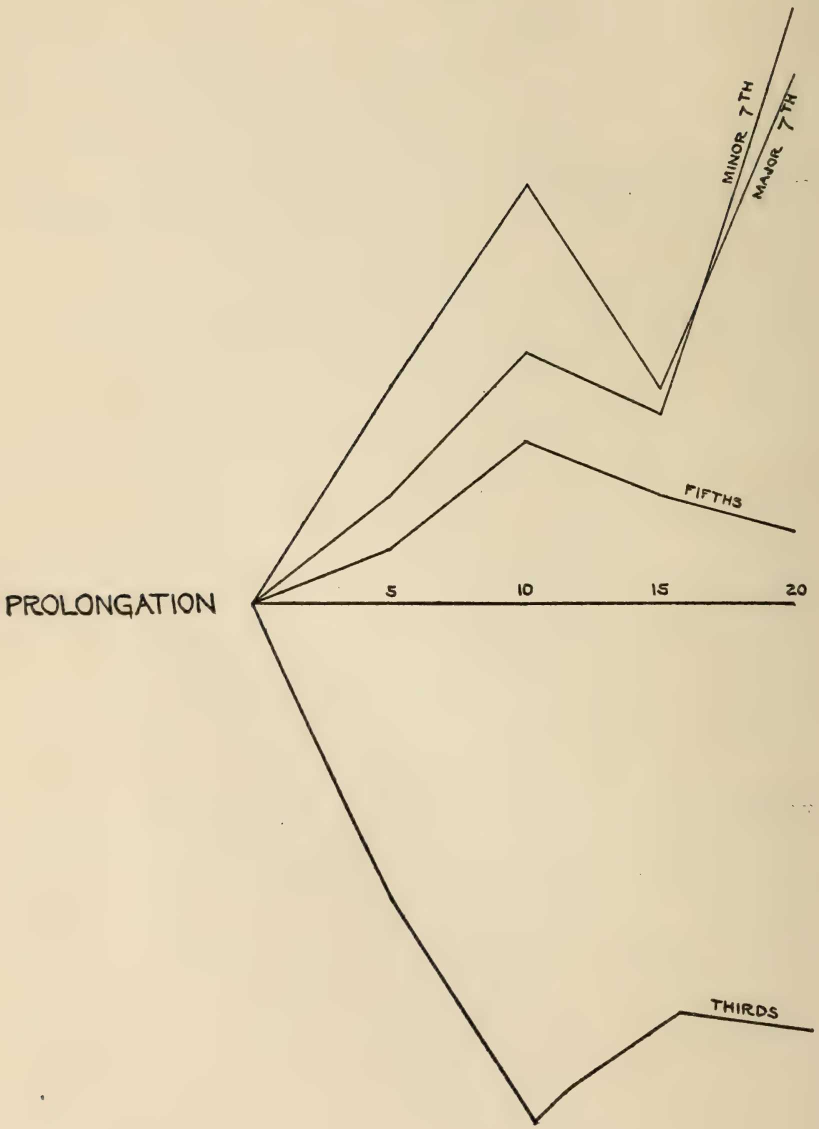


TABLE XVIII
SUBJECT G
CONSISTENCY .92
DISSONANCE RANK 1

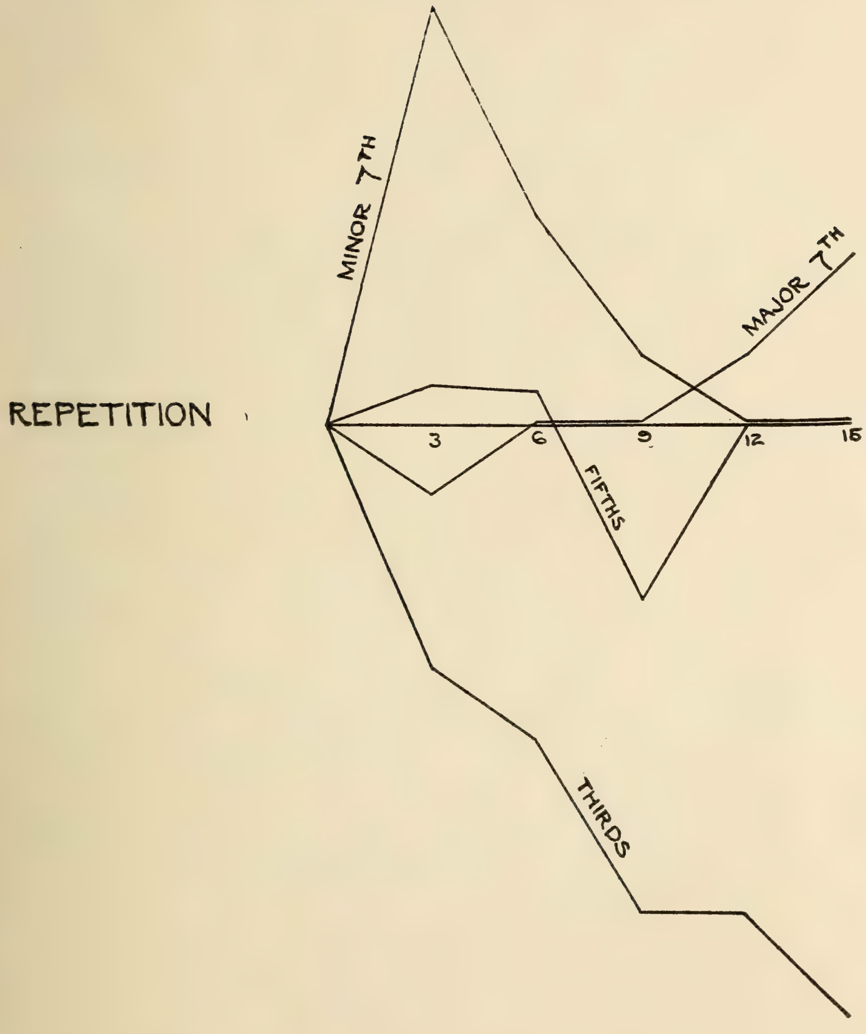
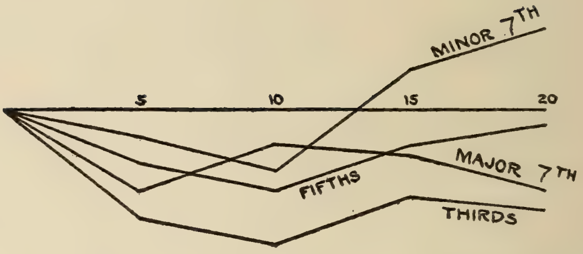


TABLE XIX
SUBJECT K

CONSISTENCY .86
DISSONANCE RANK 6


PROLONGATION

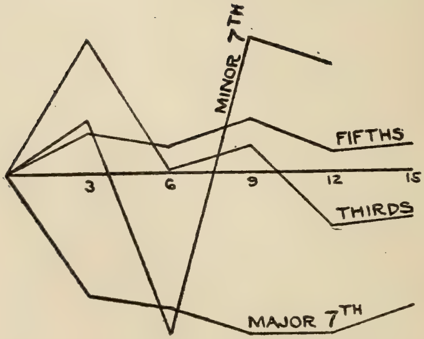
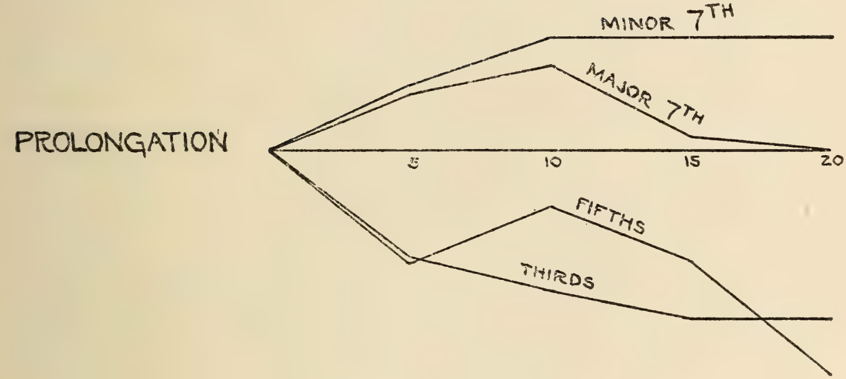

REPETITION

TABLE XX
SUBJECT L
CONSISTENCY .78
DISSONANCE RANK 2



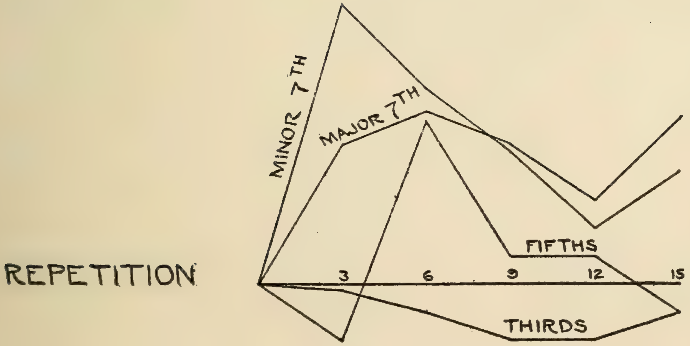


TABLE XXI

SUBJECT M
CONSISTENCY .84
DISSONANCE RANK 8

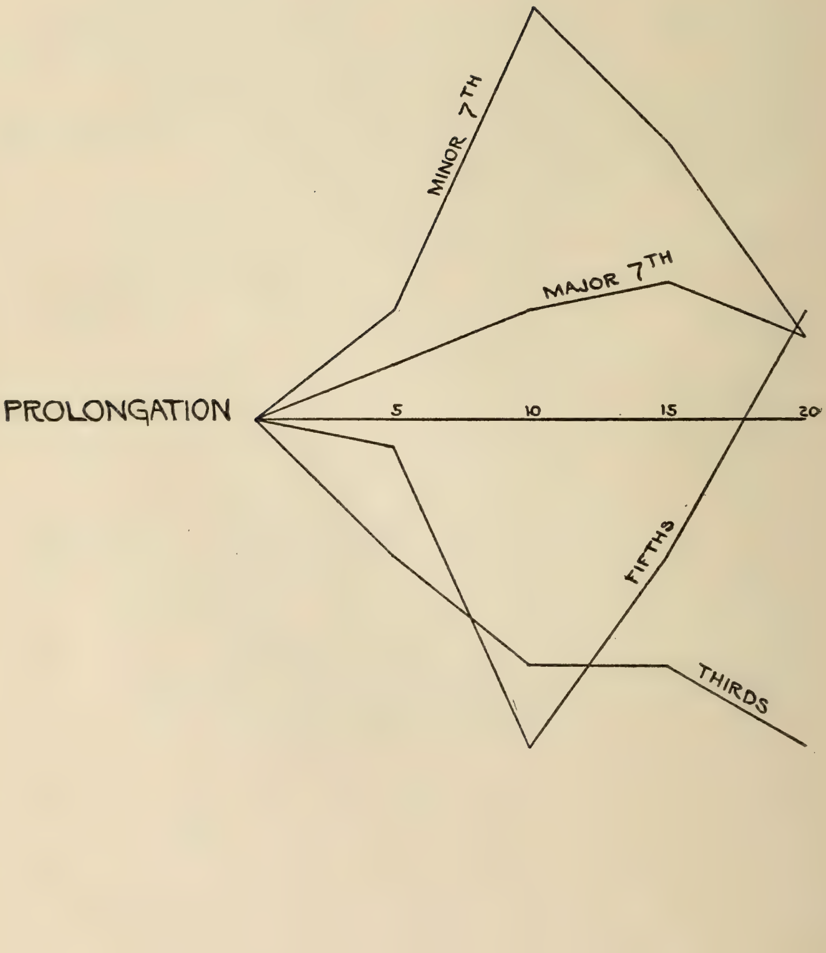


TABLE XXII
SUBJECT M
CONSISTENCY .84
DISSONANCE RANK 8

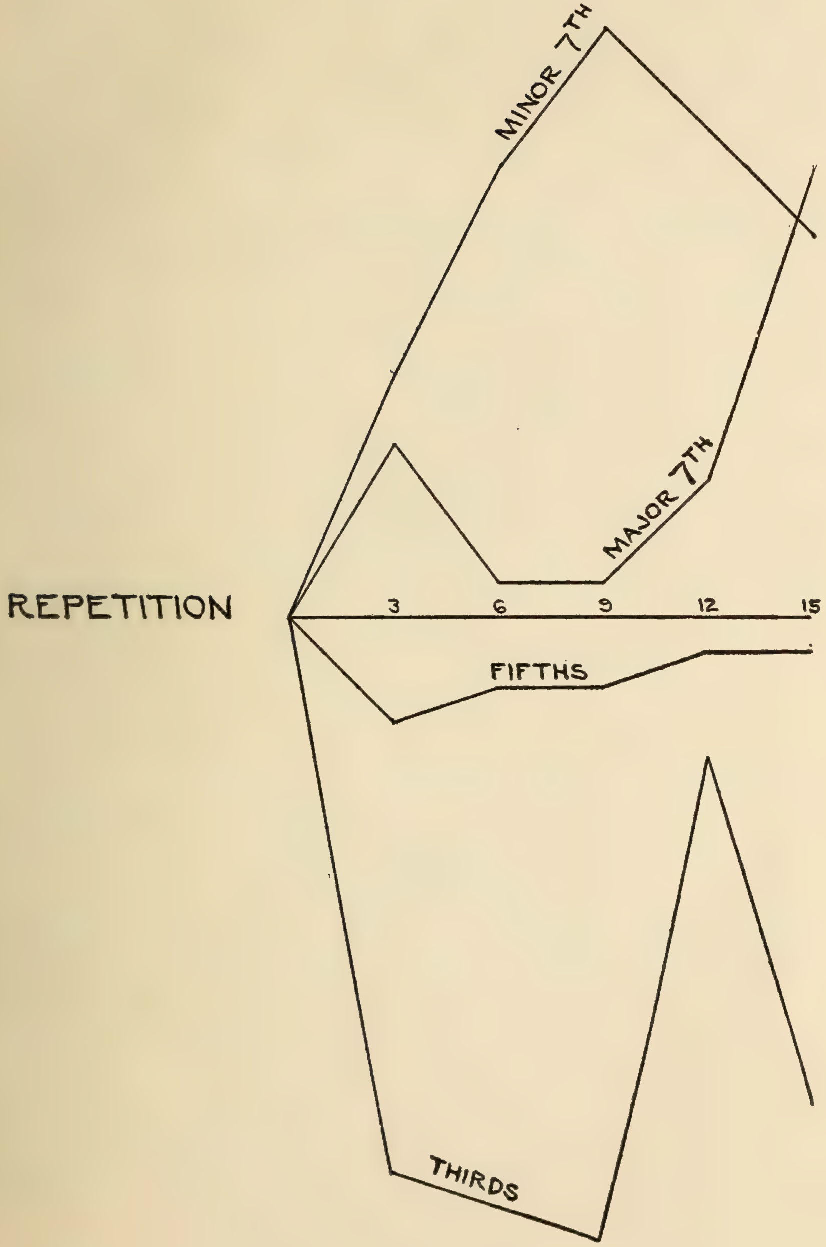


TABLE XXIII

SUBJECT S
CONSISTENCY .88
DISSONANCE RANK 9

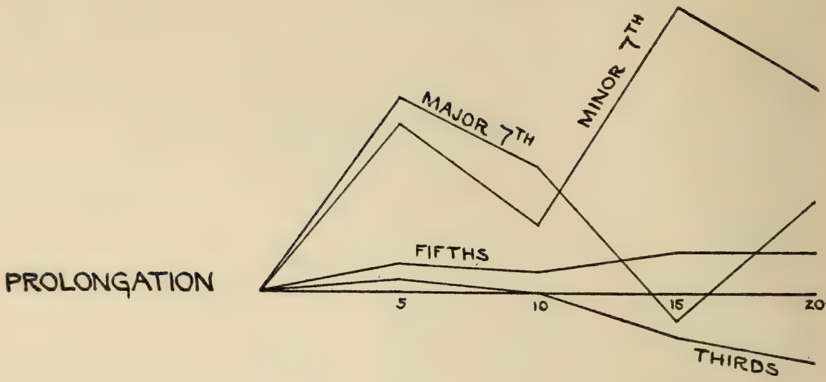


TABLE XXIV

SUBJECT R
CONSISTENCY 71
DISSONANCE RANK 3

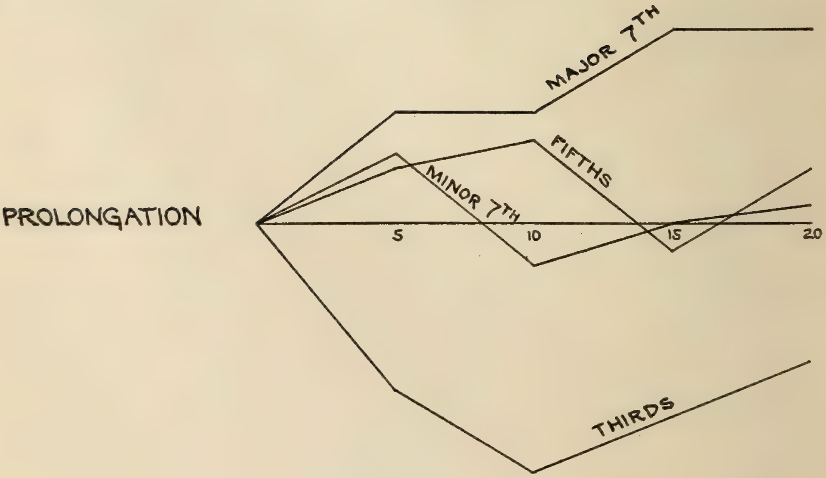
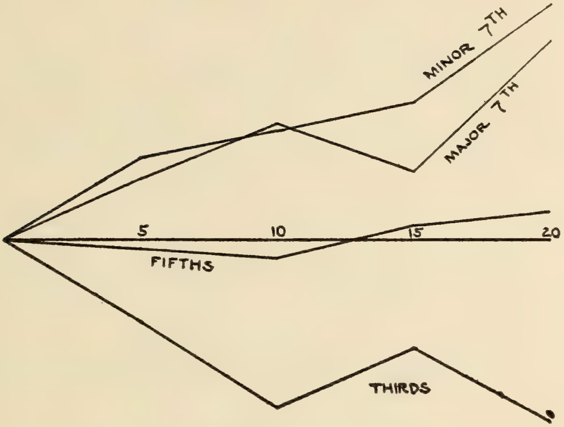


TABLE XXV
AVERAGE OF ALL SUBJECTS

PROLONGATION



REPETITION

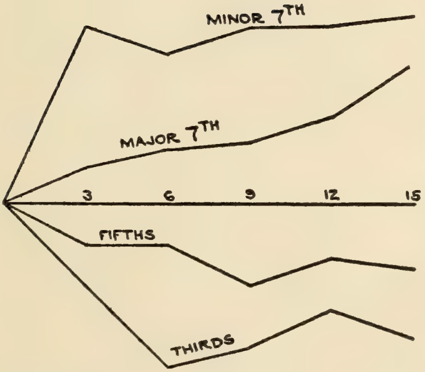


TABLE XXVI
AVERAGE OF SEVEN MOST CONSISTENT SUBJECTS

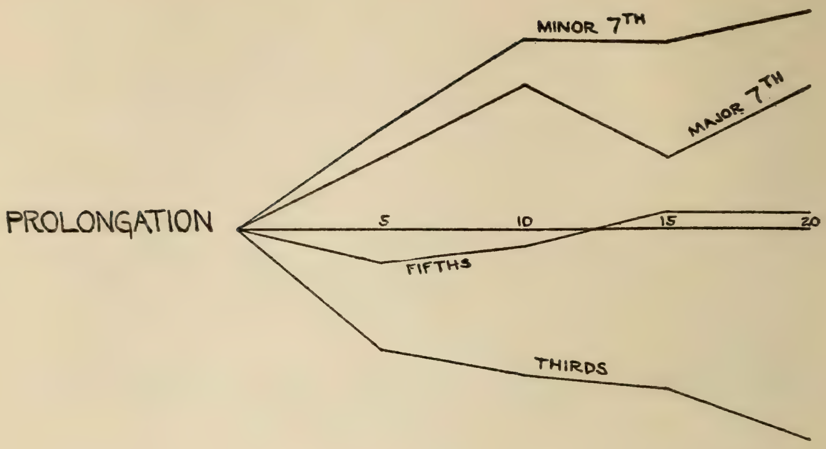
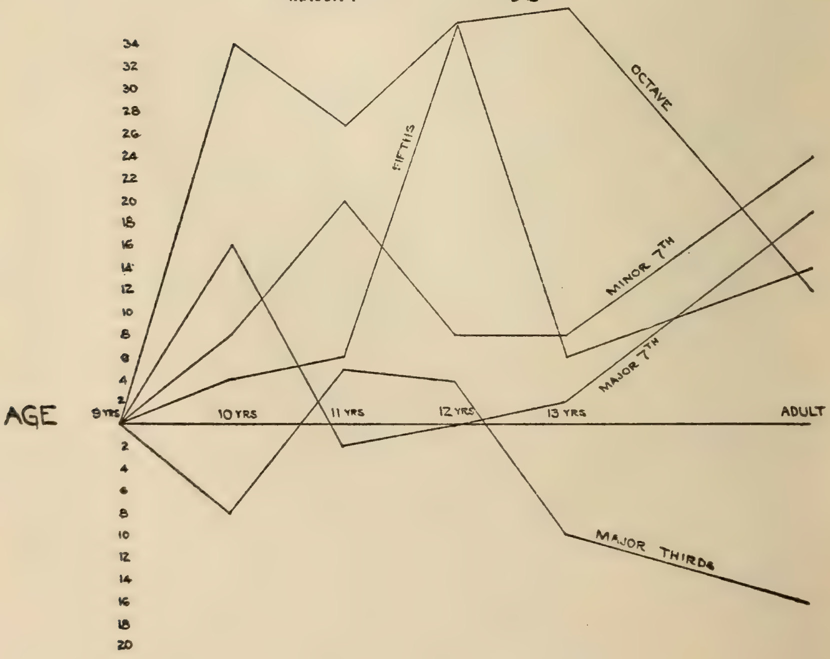


TABLE XXVII
ONTOGENETIC DEVELOPMENT OF INTERVAL PREFERENCES FROM RESULTS OF C.W. VALENTINE, GIRLS 9-14 YRS. AND WOMEN

INITIAL VALUE OF OCTAVE AT AGE OF 9 = +14
" " " FIFTH " " " " = +2
" " " THIRD " " " " = +52
" " " MINOR 7TH " " " " = -44
" " " MAJOR 7TH " " " " = -58



Vol. XVII
No. 3

PSYCHOLOGICAL REVIEW PUBLICATIONS

October, 1914
Whole No. 74

THE
Psychological Monographs

EDITED BY

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF PENNSYLVANIA

The Influence of Distractions on the Formation of Judgments in Lifted Weight Experiments

By

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PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.);
LEIPZIG (Hospital St., 10); PARIS (76 rue de Rennes)

PRINCETON
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PRESS

STATEMENT OF PROBLEM[1]

The experiments reported here deal with the influence of distractions on the formation of judgments on lifted weights. These experiments afford particularly favorable conditions for such an investigation. The objective conditions can be kept fairly constant and former experience has shown that the subjective conditions undergo but little variation. These experiments were made in the first time order, that is, the standard weight was always lifted first and the comparison weight second. The distractions used divide the experiments into two groups. In the first group the distraction was a continuous auditory stimulus produced by an electric buzzer when the comparison weight was lifted. In the second group it was the counting of discrete sounds given during the lifting of either the standard or the comparison weight. In both groups the distraction was given only on the standard or comparison weight because we wished to have experiments that would differ as little as possible from series in which no kind of distraction was used. This made it undesirable to have the distraction on both weights of a pair in one series while we did not have it on either weight in the other series. Another important consideration is that the subjective conditions be the same for both series and this can not be guaranteed if the experiments are carried on at different times. For this reason we decided that the experiments in the two series should be carried on simultaneously and that experiments with the distraction should alternate with those free from it.

The first arrangement makes use of what is called the single task method or the method of distraction and the second is the double task or the method of the distribution of attention.[2]

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[1] I wish to thank Professor Witmer for placing at my disposal the facilities of the laboratory, Assistant-Professor Twitmyer for invaluable aid in the construction of the apparatus and Assistant-Professor Urban for the suggestion of the problem.

[2] L. R. Geissler. Am. J. of Psy. Vol. XX, page 487.

These two methods differ fundamentally in the way in which the subject's attitude is affected and a characterization of mental processes under different conditions may be obtained. In the first arrangement we require the subject to perform some particular operation and then interfere with that operation by another stimulus. The attitude of the subject is affected by a controllable stimulus. In the second case the attitude is directly affected, the subject is required to carry on two mental processes simultaneously and we may suppose that not only is the mental attitude of the subject different but also that the processes influence each other. Disregarding the complex nature of this interconnection we may consider the difference in the estimation of the weight under the normal and under the complicated conditions and speak of the influence of the distraction. It is this influence that we wish to investigate.

No attempt was made to measure the distraction but care was taken that it should remain constant throughout the experiment. The formulation of our problem, namely, the influence of a distraction on the formation of a judgment, does not require a definite measure of the distraction but only requires that no variation of it take place within the investigation.

DESCRIPTION OF THE APPARATUS

The Weights

The weights used in this experiment were hollow metal cylinders, two and one-half inches in diameter and one inch high, closed at one end. In their preparation they were all made slightly lighter than the lightest weight to be used in the series and then melted solder was poured into them to such an extent that each weight was just slightly heavier than it would be when used in the experiment. Then, in order to have them exactly accurate, a sufficient amount of the material was scraped out. In previous experiments the material used for this final adjustment was parafine but this is more susceptible to hygroscopic influences than the solder. The material used is the least subject to variation from the action of the weather, temperature, etc. So little was it influenced that at a second weighing, of one half of the weights chosen in random order, made at the conclusion of the first series of 500 experiments, a variation of more than two milligrams was found in only one case and in the majority of the others a variation was not registered on the balance.

The weights being thus constant under the same conditions, these conditions must be kept constant. By using the same number of standard as comparison weights no weight is lifted more frequently than another. Thus a variation of temperature due to the contact of the warm hand is the same in every case. The absolute temperature of the weights is not such an important factor because our experiments only demand that the conditions be constant for each series of experiments. The influence of temperature could have been eliminated by using for the weights hard rubber which is near the physiological zero point. It is not, however, so anhygroscopic and the variations due to such influences are much greater than in the case of the metal. By keeping the temperature of the room the same the temperature of the metal weights is practically a constant and, therefore, a negligible factor. To sight and touch the weights were all the same, being identified by marks placed on the inner surface.

The weight chosen as a standard was 100 grams so that our choice of comparison weights was to a certain extent determined. To work with comparison stimuli that were always judged heavier or lighter than the standard might result in no influence of the distraction being shown while that influence might exist. That is, the subject might correctly judge the weight as heavier whose difference was 50 per cent greater than the standard even though there was a very strong distracting mental process. It was necessary then to find a series of comparison stimuli, the lowest and highest ones of which would not be correctly judged every time. Then it was necessary to be sure that the series used without distraction would be satisfactory for the experiment when distraction was present. A further limitation on our choice is made by the fact that for the determination of the psychometric function seven comparison stimuli are sufficient. The problem of selection is then confined to the distribution of these seven. This was done empirically; weights ranging from 88 to 106 grs. with intervals of 3 grs. were tried but it was found that for Subject I alone, whose training on experiments with lifted weights was very extensive, was this series satisfactory. The other subjects required a series whose lightest weight was 84 grs. and the heaviest 108 grs. with 4 gr. intervals. Some objection might be raised to using two different series for the different subjects but as we describe our method of calculation, it will be seen that there is no serious objection to it.

The Table

The rotating table used by Dr. Urban in his experiments on lifted weights was the starting point of the development of the rest of the apparatus for this investigation. The rotating table has two great advantages. First, the space error which is very difficult to control is entirely eliminated. For the lifting of every weight the position of the subject's hand and arm is the same. Second, experimentation is made comparatively easy and the large amount of data necessary for statistical treatment can be obtained with a fair degree of speed. The size of the table, 56 inches in diameter, was determined by the number of weights to be used. Two series of experiments in each of which seven comparison stimuli were used, had to be arranged for. Thus 28 weights must be placed on the table, 14 of these being the standard weights and 14 the two series of comparison weights. They had to be placed at a sufficient distance from each other to preclude the possibility of the subject's hand touching two weights at the same time. Figure I represents a plan of the table and the accessory apparatus. Around the outer edge of the table the weights are placed, each weight occupying a section which on the circumference measures a trifle more than six inches. The weights on those sections designated by the odd numbers are the standards and those on the even numbered sections are the comparison weights. Of these weights those lifted when there was no distraction are on sections 2, 6, 10, etc., and those of the series with distraction are on 4, 8, 12, etc. On these sections the weights of a series are placed in irregular order.

The first series of experiments required the presentation of a continuous sound at the lifting of a weight. This sound stimulus might be given by the experimenter operating the apparatus for making the sounds, but two great difficulties would have to be overcome. The work of the experimenter is, without that additional task, sufficient to keep him employed and to require him to operate the sound apparatus might increase the difficulty beyond his ability. But even though the experimenter were able to do this there would be the greater objection that it would be impossible to have the two stimuli simultaneous. The experimenter could not present the distracting stimulus at the same time as the subject lifts the weight. It is more desirable to have the actual lifting of the weight actuate the sound apparatus. To arrange for this electrically is the most obvious way but the difficulty consisted in making a current by the lifting of a weight. To overcome the difficulty a relay was used. This relay in its primary position makes connection in the circuit of which the weights form a part and which is complete when all the weights are on the table. When a weight is lifted the relay completes the buzzer circuit and a sound is given. Part of the table circuit is formed by a series of wires underneath each weight. Commencing in section 1 a wire passes into section 2 where it ends. In section 2 a wire begins and passing into section 3 ends in the same relative position as the wire ended in section 2. In like manner the series of wires is arranged through each successive section until section 28 is reached. When all the weights are in position this part of the circuit is complete, the weights acting as switches between the various wires. The wire commencing in section 1 is connected with the electric battery and buzzer as follows: The buzzer, which in the cross-section part of the drawing is shown in a megaphone, is placed directly behind the subject and is connected with a battery and the relay. This relay, with a battery, is connected with contact rings placed on the base of the table. The contacts which slide along these rings are firmly fixed to the under surface of the table and connected by wires with the binding-posts shown at 1 and 28. From 1, on the under surface of the table, there runs a wire which at the outer edge of the table passes up through it and connects with the wire beginning in section 1. The circuit is now connected with the series of wires on top of the table and the current passes directly through that series until it comes to section 28. From the end of the wire in that section, a wire, similar to that connecting binding-post 1 with section 1 and similar to the wire connecting each post with the corresponding section, passes underneath the table to post 28. From that post the current passes through the sliding contact and the contact ring to the relay. Thus, when all the weights are in position the circuit is complete, the relay forming a connecting link in it. The lifting of any weight breaks the circuit and the relay is brought in as the connecting link in the circuit for the buzzer. With this arrangement, at the lifting of any weight the buzzer would be actuated and the sound stimulus given. We wish, however, to present the distraction on certain specified weights, that is, on the comparison weights of one series. It is therefore necessary to arrange a contrivance by which at the lifting of the other weights the current is not broken. This is done in the following way. Instead of allowing the current to pass into section 1 we connect binding-post 1 with binding-post 4 thereby sending the current into section 4. Thus the current will not be broken at the lifting of any one of the weights 1, 2, or 3, the connection being made through the binding-posts. This arrangement is made throughout the entire circumference of the table and by a simple changing of the connection among the binding-posts we are able to eliminate the sound in whatever order we desire.

For the second group of experiments it was necessary to arrange a piece of apparatus that would give discrete sounds in any desired number and on any desired weight. The same kind of sound stimulus was used as in the first series, that is, an electric buzzer which had to give separate clicks. The arrangement is shown in Figure II, the lower part of which represents a section of the table, the buzzer and the battery; the upper part of which shows the apparatus for giving the sounds. The portion of the drawing marked "Elevation" shows a drum in cross-section. In this drum are six horizontal slits into which contact pieces fit. The first contact piece of each slit is shown on one side of the drum. If this drum is rotated and the circuit of the buzzer completed through each separate contact at every contact a sound will be given, the number of sounds being equal to the number of contact pieces. The current of the buzzer enters the drum at A and leaves through the contacts B. Which one of the contacts it leaves through, and therefore the number of sounds that will be given, is determined by the experimenter who has beside him a selective switch which is inserted in the circuit of the buzzer. In order to set the drum in motion the current from the table was connected, in the same way as in the first arrangement, with the relay which completed a circuit for the strong electro-magnet as shown in the drawing. This magnet, when actuated, brings into play the friction gear which rotates the drum. The lines which are marked "to motor" represent belts that come from a continuously running motor. These belts turn the wheel of the friction gear which in its normal position runs free. As long as a weight is off the table, the relay is in the circuit of the electro-magnet and this keeps the friction gear in action. When the electro-magnet ceases to act a spring brings the friction wheel to its original place and the rotation of the contact drum ceases. But the drum must be brought back to its initial position, otherwise at the next lifting of a weight the drum might have to make almost a complete revolution before the first contact would be made. The introduction of another current was sufficient for this purpose. Contact C when the drum is at rest, touches the piece of hard rubber inserted in the metal ring that rotates on and with the axis of the drum, but which is insulated from the rest of the apparatus. Contact D is always in connection with this metal ring: These two contacts are connected with the electro-magnet and the battery. While the drum is at rest no current can pass through this circuit because of the break made by the rubber insertion. When the drum commences to rotate the rubber piece glides away from contact C and the circuit is complete. This current actuates the electro-magnet until the drum has made a complete revolution and contact C again rests upon the piece of hard rubber.

A very important factor is the presentation of the distracting stimulus simultaneously with the stimulus to be judged. We do not see any way by which this could more accurately have been done than by the way used here. In order to determine exactly how far we had succeeded we measured the actual time elapsing, between the lifting of the weight and the first sound, by graphic registration. This was done for both groups of experiments by using two electro-magnetic time-markers. One was inserted in the primary or table circuit and was actuated when that circuit was complete. The other was inserted in the buzzer circuit, the marker being free while all the weights were on the table. Lifting a weight released the marker of the table circuit and a break in the line on the kymograph was registered. At the first sound the buzzer circuit was complete and the magnet of the time-marker in that circuit was actuated thus registering a break in the line which it made on the kymograph. We have now a record showing the amount of revolution of the kymograph between the lifting of the weight and the sound of the buzzer. In order to translate this amount of revolution into time units a tuning fork giving 100 vibrations per second was used. It made its record just above the records of the time-markers and by measuring the distance between the break of the two lines and counting the number of vibrations given by the tuning fork within that distance the time was determined. This measurement for the first arrangement gave in terms of σ 49.24 with a mean variation of 0.76 σ. The greatest variation was 1.24 σ, that is, the lowest time given was 48 σ and the highest was 50 σ. For the second arrangement the average time was 232.2 σ with a mean variation of 1.07 σ. The largest variation in this case was 2.42 σ. What we do in these measurements is, in the first case, to measure the latency of the magnet in the relay and in the second case to combine this with the time between the commencement of the rotation of the drum and the first contact. To one who did not see the apparatus working the second result might seem impossible. In the preparation of the apparatus I had not hoped to obtain such an accurate result but observation of the rotation showed that we had exceeded expectations. For such observations, numbering hundreds, the variations in distance from the point at which the drum was expected to stop were so small as to be in most cases unnoticeable. The numerical result is then not surprising.

The entire apparatus was very reliable. The difficulty of making the electric connections by the weights was overcome by using several strands of a very fine wire instead of one heavier strand. The total number of stops for all causes during the entire investigation was very small. In the first part of the experiments it was never necessary to stop for more than a few moments. With the second arrangement, using the very complicated apparatus it was necessary to stop for the balance of the period on three different occasions but the few other stops were for very short times.

METHOD OF PROCEDURE

The experimenters were Subjects III and IV. While Subject IV acted as observer Subject III was experimenter. For Subjects I, II, and III Subject IV was experimenter. Their work, in the first group of experiments consisted in turning the table and recording the judgments. For the records we used prepared blanks in which the judgments on each comparison weight were recorded in one line. In addition to this, the experimenter, in the second series, had to operate the selective switch to produce the number of clicks desired and to record the number reported by the subject. The number of sounds given at any particular time was unknown to the subject but it was known that the number would not exceed 6, a limit which was set by the ability of the subjects to discriminate those sounds in the time allowed. Any particular number was given on every comparison weight the same number of times. To make this possible the experimenter made out slips which indicated the number of clicks for every comparison weight in any round of the table. These slips he followed in setting the selective switch. The position of the operator is shown at one side of the table in Figures I and II.

On the opposite side of the table is the position of the subject who is seated in a chair provided with an adjustable arm-rest. His hand extends beyond the arm-rest, through a screen which shuts off his view of the table, and is in a position to lift the weight. The lifting of the weights is regulated by a metronome giving 92 beats a minute with a bell ringing every fourth beat. At the first beat, when the bell rings, the subject allows his hand to drop toward the table and grasps the weight, at the second beat he raises the weight, at the third beat he replaces it on the table and at the fourth beat he raises his hand. He is then in the same position as at the beginning of the experiment and is ready for the next lifting. The time between replacing the weight and the next downward movement is sufficient for the experimenter to turn the table and bring the next weight directly beneath the hand of the subject. Each subject was allowed to select the manner of lifting most convenient for the formation of his judgments. A considerable number of practice experiments, made before the beginning of those recorded, enabled him to find this out. The lifting was done by simply raising the hand at the wrist and no attempt was made to regulate the excursion of the hand. The judgments given were heavier, lighter or equal, the judgment always referring to the second weight of a pair. An essential point in this method is, that the subject never knows what intensity of weight is to be given. In order to make it impossible for him to know he was never told in what order the weights were placed on the table. In addition to this, at the end of every 150 experiments the order of the weights was changed so that he could not gain any information by the lifting. In the second series a statement of the number of clicks had to be given. The instructions given to the subject were that he should give the best possible judgments on the weights. This implied that he would be passive in regard to the sound in the first series but in the second series he was instructed to count the number of clicks as accurately as possible.

That the instructions were followed well we have proof in the agreement of objective evidence with the statement of the subjects. Each subject reported that his attitude in the first group of experiments was quite passive although Subjects II and III said that anticipation of the sound was probably an incentive to rapid judgment. That attention was given mostly to the weights and that the subjects were for the most part passive to the sounds is shown by the fact that there were frequently at the beginning of a three-minute period a number of judgments made when the distraction should have been present but was not and at times none of the subjects would notice it until judgments had been given on every pair of weights on the table. This is evidence that the attention was given primarily to the judgment of the weight and the opposite result in the second part confirms this evidence. In that part all the subjects noticed the absence of the sounds even though they were omitted in only one case.

All the subjects had been well trained in laboratory experimentation but only one had been trained in experiments on lifted weights. None of the other three had done any work of this kind so it was necessary to make a long series of practice experiments before the subjects were qualified to carry on the regular work. The qualification necessary was that the subject become practically automatic in his liftings, that is, automatic to such an extent that it requires no effort to keep his movements in time with the beats of the metronome. The practice experiments for this purpose extended over six regular sessions and each subject made about 2000 preliminary judgments. When the second part of the experiments was begun each subject had to have three more practice sessions in order to learn to count the sounds at the same time that he lifted the weight.

In each session 30 judgments on every comparison weight were made. At the beginning of each session the subject lifted every weight on the table as a warming-up series as well as to put them in the right position relative to his hand. A session was divided into ten periods at the beginning of which a few experiments were made that were not recorded. These were simply for the purpose of allowing the subject to concentrate on his task. At the end of each period the subject rested approximately one and a half minutes and, the experiments of a period requiring between three and four minutes, a session lasted about an hour. Neither the experimenter nor the subject felt that period of time to be a great strain and this is a very important fact especially in reference to the subject. If he should find the work very difficult and become weary toward the end of the hour we have different conditions under which the experiment is carried on and therefore our results at the beginning and at the end of the hour are not strictly comparable. It will be necessary to determine by exact mathematical calculations whether or not such a factor is to be considered, but for two reasons we do not believe it to be important. First, for two comparison weights the coefficient of divergence was calculated and was practically unity, thus indicating that the experiments were made under the same conditions during the entire hour. Second, for the first 500 judgments the number of heavier, lighter and equal judgments was counted for each of the ten periods of a session; that is, the sum of these judgments for the first period, for the second period and so on for each period of every day was calculated. These sums show that the proportion of different judgments given on the different comparison weights is practically the same in every period.

RESULTS AND METHOD OF TREATMENT

The data of the investigation were collected in groups or series of 50 judgments on each comparison weight. These series are given in Tables 1 to 24 in the appendix. In the first column of a table are given the numbers of the series. The other columns, taken in groups of three, contain the absolute frequencies of the judgments, heavier, lighter and equal on the various comparison weights. In all, there are recorded 61,600 judgments. This is exclusive of the judgments given in the practice experiments which numbered about 12,000. The complete investigation required, then about 75,000 judgments; a number sufficient to answer the questions raised in connection with this particular problem. These judgments are divided among the three arrangements that we used in the investigation. Tables 1 to 4 give the frequencies of the different classes of judgments in the first 10 series for each subject when the judgment was made without the distraction. In Tables 5 to 8 are the corresponding frequencies of the judgments for the first 10 series when the subject was distracted by the sound stimulus. Tables 9 to 16 give the results for the experiments with and without the counting of the sounds in the six succeeding series of judgments. In these series the sound was presented simultaneously with the comparison weight. The next group of tables, from 17 to 24, contains the frequencies of the different classes of judgments for 300 experiments for each subject when the sounds were given simultaneously with the standard stimulus.

The results given in these different series, while not identical for the same comparison weight, are sufficiently consistent to be regarded as empirical determinations of probabilities. Because of this fact the results may be treated by the method of Constant Stimuli.[1] This method resolves itself into the determination of two quantities which are usually designated by the letters h and S, of which the latter refers to the difference threshold and the former to the measure of precision of judgment. S_1 refers to the lower threshold and S_2 to the upper threshold while h_1 and h_2, determining the rapidity of rise of the curves of the psychometric function, refer to the lighter and heavier judgments respectively. The results for every comparison stimulus furnish observation equations of the following type: $h(X - S) = \gamma(P)$, where X is the value of the comparison stimulus and γ (γ is the t of Titchener's terminology) the value given in the $\Phi(\gamma)$ formula. The values of X are given in the comparison weights used. The values γ are found in the fundamental table for the method of right and wrong cases (Urban p. 368). Each equation has a weight (P) which is given in the table of weights (Urban p. 371). If the number of comparison stimuli is greater than two the system of equations for the determination of the values h and S is over-determined, there being required only two equations for the determination of two unknowns. The most advantageous way of treating such an over-determined system, Urban has shown, is by the Method of Least Squares where the normal equations for the two values are as follows:

$$[X\,X\,P]\,h - [X\,P]\,C = [X\,P]$$
$$-[XP]h + [P]C = -[\gamma P]$$

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[1] G. E. Muller; Gesichtspunkte und Tatsachen der psychologischen Methodik. E. B. Titchener; Quantitative Psychology. W. Wirth; Psychophysik. F. M. Urban; Die psychophysischen Massmethoden als Grundlagen empirischer Messungen; Archiv für die gesamte Psychologie XVI Bd.

The value C is used in the equation for the sake of simplicity, it really being the product of h and S of the observation equations. The use of the square brackets in these equations means that the sum of the products of these values is to be taken, that is, the sum for every observation equation. The values are the comparison weight X; γ, the value found in the fundamental table for the p, or probability, of the particular X; and P, the weight of the observation equation for that same X. The demonstration is based on the fact that the numbers of the relative frequencies of the different judgments under constant conditions have the character of empirical determinations of unknown probabilities. From this it follows that the errors in these determinations are given by Bernoulli's theorem, that is, that they are distributed according to the $\Phi(\gamma)$ function.

The setting up and the solution of the normal equations is not difficult but it must be arranged systematically because of the possibility of errors in the long computation involved. The amount of work is so great that the method of constant stimuli was for a long time an entirely impractical method. That we have been able to use it in this investigation is due to the method and tables furnished by Urban.[2] For this method it is only necessary that the intervals between the various comparison stimuli be the same. We then arbitrarily substitute the value 0 for the middle comparison weight and the values 1, 2, 3, etc., and —1, —2, —3, etc., for the comparison stimuli above and below this middle one. Our computations are thus reduced to the very simple ones of multiplying these small numbers and the corresponding values of γ and P. But the products to be added in the terms of the normal equations being once determined for each value of p (the n in the terminology of Titchener) are determined for all cases. These products Urban gives in the table of pages 240 and 241 of the article referred to. In forming our tables for the calculation of the values h and S it is necessary to transcribe the products for the various probabilities, compute their sum and use these sums in the normal equations. The resulting h and S which we call h′ and S′ are not the value we would have gotten by the longer calculation. In order to determine the true values a very simple calculation is necessary. The value h′ is divided by the number of units between two successive comparison stimuli and the result is the true h. The value c is obtained by multiplying the value h by the comparison stimulus taken as zero and adding to the product the value c′. To show the actual amount of work required a sample table is here appended.

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[2] F. M. Urban; Hilfstabellen für die Konstanz Methode; Archiv für die gesamte Psychologie, Bd. XXIV.

Solution of the Normal Equations for h and S. Subject I; Series 11; With the Distraction; Lighter Judgments.

X	p	P	γP	XP	XXP	XγP
3	0.02	0.1881	0.2732	0.5644	1.6931	0.8195
2	0.02	0.1881	0.2732	0.3762	0.7525	0.5463
1	0.18	0.7327	0.4743	0.7327	0.7327	0.4743
0	0.46	0.9964	0.0707			
—1	0.60	0.9768	—0.1750	—0.9768	0.9768	0.1750
—2	0.78	0.8025	—0.4382	—1.6051	3.2102	0.8764
—3	0.94	0.3954	—0.4346	—1.1861	3.5582	1.3039
		4.2800	0.0436	—2.0947	10.9235	4.1954
		h 10.9235	+c 2.0947	= 4.1954		
		h 2.0947	+c 4.2800	= —0.0436		
		h@	+c 4.3878	= 8.7881		
		h@	+c46.7526	= —0.4763		
			—c42.3648	= 9.2644		
			c′ =	—0.2187		
			h′ =	0.4261		
			c =	13.5553		
			h =	0.1420		
			S =	95.46		

In the table, X is the comparison stimulus used in this auxiliary method of calculation; p is the probability, or the relative frequency, of the judgment; P is the coefficient of weight and γ is the value gotten in the fundamental table. The entire work of making out and computing one of these equations requires less than half an hour.

DISCUSSION OF RESULTS

Tables 33 to 44 in the appendix contain the results obtained by this method of calculation from the original data given in Tables 1 to 24. These tables are arranged in the same order of subjects and series. The first column of each table gives the number of the series. The columns designated by h_1 and h_2 contain the coefficient of precision for the lighter and heavier judgments respectively. The columns containing the upper and lower thresholds are marked S_1 and S_2. The difference between these two thresholds is called the "interval of uncertainty" and is given in the column so marked. The last column contains the value designated "point of subjective equality." This quantity is defined by $\frac{c_1 + c_2}{h_1 + h_2}$ and closely approximates the average of S_1 and S_2 owing to the small difference between h_1 and h_2. With the exception of the first column, each is divided into two sections, one section marked with a minus and the other with a plus sign. These signs mean that the results given in the divisions so marked are for the series without and with the distraction. The last line in each table is the average of the results given in the corresponding columns.

In order to have a more direct comparison we have collected these final averages in separate tables. Table 49 contains the averages of the first ten series for each subject as well as a general average of the results for the four subjects. From the general average we find that the values h_1 and h_2 are greater in the series in which the sound stimulus was given than in the series in which there was no interference. In the same series the interval of uncertainty is smaller and the point of subjective equality is lower. The averages of the various subjects show practically the same fact as this general average. The h_1 with sound is in every case larger than the h_1 without the sound. The same thing is true in 3 cases of the h_2 while one case is negative. In the results of the individual series for the different subjects we find the following. Subject I (Table 33) has 19 results in which the h is larger in the series with the sound than in the series without it. That is, only one case does not conform to the general average. Subject II (Table 34) gives 13 results in which the h's in the series with the sound are greater and 7 where they are smaller. Subject III (Table 35) shows a similar result. In 15 cases the h with the sound is larger while in 5 cases the opposite holds. For Subject IV the cases are equally divided, 10 of the h's with sound being larger and 10 smaller than the corresponding h without the sound.

The results with which we have been dealing are those of the first 500 experiments, that is, those in which a continuous sound was given on the comparison weight. It is interesting to compare directly with these the values of the h in the two arrangements of the second group. The averages of the subjects and the general average for these two arrangements are given in Tables 50 and 51. Table 50 contains the averages for the group with the distraction on the comparison weight. The general average of this table reveals the same fact as was pointed out in the discussion of Table 49. The h in each case is larger in the series with distraction. Six of the individual averages agree, while only two disagree, with the general average. The individual series of the subjects, as given in Tables 37 to 40, show a like correspondence with the general average. For Subject I there are nine cases of agreement and three of disagreement; for Subject II there are four results agreeing and eight disagreeing with the general avearge. For Subject III ten results show a like preponderance of the h with the sound, two show the opposite. Subject IV gives an equal division; six cases of agreement and six of disagreement with the general average.

In Table 51 we have these values for the 500 experiments in which the subject counted the sounds as they were given with the standard weight. In this group a similar result is found; h_1 and h_2 are larger in the general average of the series with the sound and the relation of the individual averages to the general average is similar to that of the previous series, but the distribution of the cases is somewhat different. There are seven cases of agreement and only one disagreement with the general average. In the individual series Subject I gives nine results that agree with the general average, two where there is disagreement and one case in which the h without the sound and the h with the sound are exactly the same. Subjects II and III give identical results; six cases for each have an h greater with the distraction than without. Subject IV has eight results agreeing and four disagreeing with the general average.

So far we have dealt only with the value h. Our next consideration is the interval of uncertainty. The general averages for the three groups given in Tables 49, 50 and 51 show the interval smaller in the series with distraction. The averages for the individual subjects agree in every case with the general average, that is, the interval of uncertainty in the series with the sound is smaller than in the other series. For the individual cases we find practically the same result. In series 1 to 10 the total, for the four subjects, of agreements with the general average is 33 and of disagreements only seven. In series 11 to 16 there are 18 cases where the interval in the distraction series is smaller and 6 where it is larger than the corresponding interval without the distraction. Series 17 to 22 give a like result; in 19 cases the interval is smaller and in 5 larger with the distraction.

The general averages of the point of subjective equality are different in the two series of the two groups. We find that in Table 49 the general average is smaller in the series with the sound. However, only two of the four subjects give results that agree with this general average. The individual series of the subjects are divided in a similar way. Subject I in each of the ten cases gives a lower point of subjective equality in the series with the sound. For Subject II there are seven results where the point is lower and three where it is higher. The results for Subject III are directly opposite to those of Subject I. In this case the point of subjective equality for all series with the sound is higher than for the corresponding series without the sound. In the results for subject IV there are only four cases where the lower point is registered in the series with the sound. The total of these cases for the four subjects is 21 that agree with the general average and 19 that disagree with it.

For the first part of the second arrangement the results are not so ambiguous. The general average, as given in Table 50, shows the same feature as the general average of the preceding group, that is, the point of subjective equality is lower for the series with the distraction. The averages for the individual subjects give three agreements with this general average while there is only one disagreement. The individual series of the subjects show a corresponding preponderance of cases that agree with the general average. Subject I has the same number of agreements as disagreements. The results for Subject II are quite the opposite of the general average, giving only one case in six that agrees with it. Subjects III and IV give results that correspond to the general average, the former giving six cases that agree, the latter five that agree and only one that disagrees. In the total for the subjects there are fifteen results that agree with the general average and nine that disagree.

The general averages of the point of subjective equality in the two series of the other part of the second arrangement are practically identical. The averages for the individual subjects give, as would be expected, two agreements and two disagreements with this general average. Of the individual cases for the various subjects we have the same thing to relate. Thirteen of the cases agree with this general average and 11 disagree.

A clearer expression of the results may be gotten in the curves of the *psychometric functions* for the different arrangements. These curves, given in Charts 1-12, are plotted from the results of the various groups of experiments. That is, Charts 1-4 are plotted from the results of the first 500 experiments for each subject, Charts 5-8 from the experiments with discrete sounds on the comparison weight and Charts 9-12 from the experiments with the sounds on the standard weight. These curves give the value of the calculated p, or probability, for each value of the comparison stimulus. The calculated values of the probabilities are not identical with the observed values but Urban[1] has shown that the values of p in the $\Phi(\gamma)$ formula by which these curves are calculated agree most closely with the observed values. The calculation by which is determined the points of a curve which correspond to the comparison stimuli, is simply the solution for p in the following equation: $p = \frac{1}{2}[1 + \Phi(\gamma)]$. The value of γ we get in the observation equations for each comparison weight and the value $\Phi'(\gamma)$ we get in Bruns'[2] table for the $\Phi(\gamma)$ function.

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[1] F. M. Urban; Psychophysischen Massmethoden als Grundlagen empirischer Messungen.

[2] H. Bruns; Wahrscheinlichkeitsrechnung und Kollektivmasslehre.

The full lines in these charts are the curves of the psychometric function for the series without the distraction. The broken lines are the curves for the series with distraction. In sixteen cases the latter curve ends nearer unity than the former, in four cases the two curves end at the same point, and in four the curve for the series without distraction is higher. These facts are in strict accord with the statements in reference to the mathematical results. The interval of uncertainty which includes the comparison stimuli on which the p for the lighter and heavier judgments does not exceed the value ½, is in nine cases clearly smaller for the distraction series. The points of subjective equality, the points of intersection of the curves for the lighter and heavier judgments, given in these curves likewise correspond to the results of the mathematical calculations. For the first group of experiments, in two cases the point is nearer the lightest comparison weight, and in two cases nearer the heaviest comparison weight for the distraction series. In the second group three of the curves of the distraction series are shifted toward the left, while one is further to the right, that is, in the direction of the heavier comparison weights. The results of the third group show points further to the left in two cases, further to the right in one and one case where the points for the two series practically coincide.

Briefly summarizing these results: the coefficient of precision is larger for the judgments in the series accompanied by a distraction; the corresponding interval of uncertainty is smaller. The point of subjective equality is lower in the series where the distraction was given simultaneously wiith the comparison weight, but in the series with the distraction on the standard weight it is the same as in the series free from distraction.

With such results we must now ask the reason for them; and we take first the values of the h and the interval of uncertainty. For both these the result is a contradiction of what we expect. The h is a measure of precision of judgment and instead of it decreasing when we have a distraction, it shows quite a marked increase. We would expect that the effect of a distraction would be to decrease the ability to form a correct judgment although

Titchener[3] says "It has been shown, experimentally, that we attend best under a slight distraction; maximal clearness requires a little 'effort', as we say, for its attainment." It seems that in the result of our experiments we have a proof of this statement. The judgment that is made when the mental process is interfered with by other factors is more accurate than one made when there is not such a distraction. On the other hand, it is quite possible that a very short length of time is required for a correct judgment of these comparison stimuli and that the first impressions are most reliable for the formation of a judgment. Thus the sound distraction, rather than being a distraction or interfering with the primary processes of judgment simply compels a greater rapidity of work. In other words, distraction does not mean that we carry on our work in a more satisfactory way, it simply means in this case that the subject is compelled in a short time to give a judgment on the basis of elements which would be neglected if a longer time were allowed for the judgment. In support of this statement we may refer to the series of experiments carried on by Dr. Urban[4] in which he deals with the equality judgments (page 99). In this series of experiments the subjects were required to guess whether the weights which they had judged as equal were heavier or lighter. The guesses were, in a majority of the cases, a correct estimation of the relative intensity of the weights. Then, as Dr. Urban says (page 100), "There exists therefore more than an even probability that a guess judgment will be correct. This seems to indicate that the subject in giving a judgment on the equality of two stimuli is liable to overlook certain data of the sensation which would be sufficient to decide his judgment." In our experiments no guess judgments were allowed, but the same principle is involved as in the investigation reported by Dr. Urban. "The data of the sensation sufficient to decide his judgment" were not prominent in consciousness but the "guess" judgment was based on these. If, however, a record of the equality judgments had been kept and at the conclusion of an hour's work the subject had been required to guess, his judgments would most likely have been as often wrong as right. In other words the elements on which the judgment was based disappeared. Instead of requiring the subject to guess we had an experimental arrangement that compelled the same kind of judgment. The distraction compelled the subjects to decide, before these factors were neglected, what judgment they would give.

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[3] E. B. Titchener; The Psychology of Feeling and Attention, page 203.

[4] F. M. Urban; The Application of Statistical Methods to the Problems of Psychophysics.

The problem whether or not the greater precision of judgment is due to central or peripheral factors is here met. Our conclusion is that the explanation is to be found in the central factors. In other words the sensations upon which judgments are given are the same in the series with the sound as in the series without the sound. The influencing factor, then, must be found in the attitude with which the subject faces his problem. Although the subjects reported generally that in the first arrangement they were not prepared for the sound, it came at such regular intervals, that it seems most likely that the subjects were aware when the distraction would be given. This does not contradict the statement (on p. 13) that the subjects paid attention primarily to the judging of the weights and were passive to the sounds. The fact that they did not miss the sound in the first of a period shows that it had not a prominent part in consciousness but when the experiment was in progress we are quite sure that the subjects were conscious that the sound would be given. Being aware of it, their attitude was that of giving a judgment as quickly as possible and so allowing as little time as possible for the distraction to influence the judgment. We are reminded in this connection of the statement by Fechner that "Grey paper is not brighter the harder we look at it." Although the statement may be in a certain sense true we are not prepared to admit all that it seems to imply. We do not believe that the grey paper is not clearer in consciousness when we look harder at it. In other words, while the sensations may remain the same, their relative value in consciousness changes. The grey paper, as we look at it, becomes a prominent factor in consciousness. In the same way, the sensations produced in lifting the weight become more prominent factors in consciousness when the subject is prepared by the thought of the distraction than when he experiments in the ordinary attitude. We have then a solution of our results in terms of psychical factors, that is, in terms of attention. The subject when compelled for a brief time to give the greatest amount of attention to the judgment makes more consistent judgments than when experimenting without that brief maximal effort.

The problem presented by the decrease in the size of the interval of uncertainty is a very interesting one. This interval which gives the limits of the comparison stimuli on which the probability of a heaver and lighter judgment does not exceed ½, is a direct measure of the sensitivity of the subject. When there is a decrease in the interval it means there is an increase in sensitivity. We may connect this fact with the one previously mentioned, namely, that when a short time is allowed for the judgments the accuracy increases and the explanation is fundamentally the same as the explanation of the increase in the value h. Part of this increase in accuracy is seen in the smaller number of equality judgments given and the interval of uncertainty is directly proportional to the number of equality judgments. The area of the curve of the psychometric function for the equality judgments is equal to an area determined by the size of the interval of uncertainty. This interval, giving a measure of the sensitivity of the subject, is the distance between the points 0.5 on the curves for the lighter and heavier judgments. Constructing a rectangle, with lines passing through these points perpendicular to the abscissae as sides and the zero and unit abscissae as the two ends, we have an area equal to the area of the curve of the equality judgments. Decrease the number of equality judgments and the interval of uncertainty decreases. So our explanation of this decrease is based on the fact that a heavier or a lighter judgment is given in cases where an equality judgment would be given if the subject had more time for the formation of a judgment. The formation of a judgment rapidly due to the distraction accounts for the smaller interval of uncertainty.

The question of the point of subjective equality is one where we come in direct contact with the problem of the increase or decrease of sensation due to distraction. We have our answer to the question whether or not distraction has any influence on the intensity of sensations. This point of subjective equality gives the weight which is subjectively equal to the standard weight. If in the two series of experiments this value were the same, we would say that there had been no more influence on the judgments than that already mentioned. But the fact is that this point of subjective equality is not the same for the two series of experiments except in one arrangement. The general averages for the first arrangement are, as given in Table 49, 97.38 and 96.99; the latter being the point of subjective equality for the series in which the sound distraction was given. Our conclusion is, that there has been an overestimation of the weight caused by the distraction. Our conclusion from this general average is not in agreement with the statement given by Titchener,[5] "It is clear, then, that strong as well as weak sounds are intensified by attention, or, if you prefer the negative statement, are reduced in intensity by distraction."

Strong support for our conclusion is not given in the results for the individual series but for this fact we think we have a satisfactory explanation. Subjects I and II give results that agree quite markedly with the general average. Subjects III and IV give opposite results. The total of the cases of these two subjects which disagree with the general average is 16, while in only 4 cases is there an agreement with the general average. This result, we believe, is due to the conditions under which the experiments were carried on. It was impossible to make arrangements for these experiments at any other time than late in the evening when both subjects had become somewhat fatigued with the day's work. We believe that it is this condition that makes the total of agreements and disagreements of the individual series with the general average so nearly alike. Further evidence for this conclusion we have in the results gotten in the second arrangement during which Subjects III and IV experimented in the early part of the day. We find that, in that arrangement, the outcome

[5] E. B. Titchener; The Psychology of Feeling and Attention, page 218.

corresponds closely to the outcome for Subjects I and II. In view of these facts we must conclude that the distraction causes an overestimation of the second weight.

A comparison of the results given in the three different arrangements reveals some interesting features. In the three arrangements the h is larger in the series with distraction than in the other; and the interval of uncertainty is smaller. It is the point of subjective equality that reveals the most interesting fact. In the first two arrangements this is quite noticeably lower in the series with the sound; but in arrangement 3 the results for the two series are practically identical. The reason for this may be found in a memory image of the standard weight. In arrangement 3 the sound distraction was present on the standard stimulus and of this standard stimulus, which was lifted a great number of times, a general memory had been built up so that the distraction on it would not increase its intensity as it apparently has done in the case of the comparison weights for which no such general memory could be postulated. One difficulty to be faced in this explanation is that the coefficient of precision shows the same phenomenon in this third arrangement. But the difficulty is not insurmountable because of two facts, one of which is that the judgments are made under similar conditions as in the first two series and the other is the fact which we mention in the following paragraph in reference to the influence of the distraction on the judgments made when no distraction was present.

No attempt was made in our investigation to determine whether or not the distraction persists and influences the judgments even when it is not present. This might have been done by making a series of experiments at a different time when no distraction would be given, the results being compared with the results of the series without distraction in these experiments. Some information on the question may be gotten by a comparison of the results obtained by Subject I in this investigation with results gotten by the same subject in an investigation[6] carried on in the early part of the year 1912. The results of this earlier investigation were calculated in series of 50 judgments on each comparison weight so as to be directly comparable to the results in this investigation. The results are given in Table 52. In the first line is indicated the value given in the corresponding columns; the second line gives the year of the investigation. In the third horizontal space are the results referred to in the first column as series. Series 1 gives the average result of the first ten series of 1912 and the first ten series of 1913 without the distraction. Series 2 is the average of series 11 to 16 for both years. Series 3 is a like result for series 17 to 22. The last line gives an average of these three series. This average shows that the measure of precision is greater for the year 1913. The three series give 5 out of 6 results greater for the same year than for the preceding. It is not to be supposed that it is due to the greater practice of the subject for this subject had carried on too large a number of experiments for such a great change in this comparatively short time. Rather is the difference to be accounted for by the presence of the distraction in the related series with distraction. This distraction not only causes a more rapid judgment on the weights with which it is given but also on the other weights. This explanation is consistent with the explanation given in regard to the size of the h. In the large number of experiments made the subject forms the judgment rapidly many times when the distraction is not given and the result is a greater number of lighter and heavier judgments. It is only reasonable to assume that that influence would be shown more markedly on the pair where the distraction is present even though it be on the standard weight, than in the judgment on the pair with which there is no distraction. Had we found no influence of the distraction in the series carried on simultaneously with distraction series our conclusions would still hold. When there has been an influence, but that influence has been to lessen the difference on which we based our conclusions, our position is made even more tenable.

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[6] S. W. Fernberger; Comparison of Method of Constant Stimuli and Method of Just Perceptible Differences.

We have stated that the lowering of the point of subjective equality in the series when a distraction is given is due to the distraction. That is, when there is a decrease of attention, we have, instead of a decrease, an increase, in the prominence of the sensations. But the query is raised whether this phenomenon could not be just as well explained by a particular "form of attention." In a criticism of some experiments reported by Münsterberg[7] Miss Hamlin[8] states that his results were due to "expectant attention." The subject was prepared for a stimulus of greater intensity and the consequence was that he underestimated the stimulus given. This criticism seems to imply that there is in the attitude of the subject some factor similar to the factor producing the size-weight illusion. Instead of visual perception being the determining factor in this experiment there was a certain adjustment of the organs involved in lifting the weights. The muscles of the hand and arm are inervated in preparation for a smaller weight than that which is really lifted. It is necessary that it should be a smaller weight because the second weight is judged heavier in this series with distraction than in the ordinary normal series. It is not then an expectation of a greater weight but an expectation of a smaller weight that would lead to this difference in the position of the point of subjective equality. With such an explanation we cannot be very well satisfied when we consider the conditions under which the experiments were conducted. We have shown (see p. 13) that the subject never knew what intensity of weight he would lift and it is essential that he know this if there is to be the same adjustment of the arm and hand muscles relative to the weight lifted.

Another explanation of the difference is given in terms of adaptation. The subject having lifted one weight adjusts his hand for the lifting of a certain weight. This adjustment is so made that an overestimation of the second weight results. In this way the time error might be explained and the greater overestimation of the second weight in the distraction series is due to a greater adaptation of the hand and arm muscles. To this explanation we have one very serious objection. It is that the greater adaptation does not hold for the lighter weights of the series. More lighter judgments are given on the light weights in the distraction than in the normal series. This is shown in the curves of the psychometric functions (Charts 1-12). In eight cases out of twelve the curve for the lighter judgments in the distraction series begins higher than the curve for the other series. If the adaptation explanation were valid this would not be true for we would have a regular overestimation of all the weights corresponding to the constant overestimation which we call the time error.

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[7] H. Münsterberg; Psychological Review, vol. I, page 39 ff.

[8] Alice J. Hamlin; American Journal of Psychology, vol. VIII, page 66.

COUNTING NUMBERS

We now wish to determine the accuracy with which the various subjects counted the sounds given in the second part of our investigation. The reason for the determination is to give an answer to the question whether or not there was a second reliable mental operation being carried on by the subject. It might be that the subject gave all attention to the judgment of the weights, his count of the sounds being very unreliable. This would be shown by the number of errors in the counts. These errors are given in Table 53. In the first column the subjects are arranged in the order of their accuracy in counting. The numbers in the first line refer to the number of sounds given. In the columns corresponding to each of these numbers is recorded the number of errors made by the various subjects on those numbers. The last column gives the total errors for each subject and the last line the sum of the errors on each number. The greatest number of errors is recorded for the times when the subject was supposed to count 6. Four and five were incorrectly counted quite frequently but the difference in the ratio of errors on four and five and the ratio for five and six is quite marked. If each sound had been given under the same conditions we would not expect this result. Six, however, had an advantage over the others in that the subjects knew that there would never be more than six and therefore in that case they never made errors of over-estimation.

Of the subjects, Subject III makes the fewest number of mistakes. Subject IV has a little more than one half the number of errors made by Subject II. Subject I makes a few less than Subject II. The question, whether or not Subject III gave greater attention to the counting than the other subjects, has to be answered. For this Subject the relative number of mistakes in counting is 1 in 191 cases, that is, in the total number of experiments the subject had to count 4200 times and he made errors in only 22 of these. Subject I in the same number of experiments made 332 errors or a proportion of 1 in 13. Subjects II and IV made 1 in 11 and 1 in 20 respectively.

The difference is very marked and the question is, is the difference to be explained by a different distribution of attention. If there were no other facts to which we could appeal we might be forced to conclude that Subject III had given more attention to the sounds than the other subjects. But, not only is the accuracy of his judgments on the weights quite comparable to that of the other subjects, there are other facts which lead us to believe that the difference in accuracy of counting does not mean a different distribution of attention. These other facts are; differing sensitivity of the different subjects to sound stimuli; previous practice in sound experiments, and the specific practice of this experiment. The influence of practice in this experiment is shown in the numbers of Table 54 which gives the number of errors for each subject in each series of 50 experiments. In the first column the subjects are referred to and in the succeeding columns are the number of errors for each subject in the 12 series in which the discrete sounds were given. The last line gives the total for all the subjects. These numbers show a decrease from the first series to the last. The numbers in the early series are very large compared to the numbers in the later series; the total in the first six series being 599 and of the last six only 352 or about 68.7 per cent; and the number in the last two series is only about one-fourth the number in the first two series.

But if practice in this experiment were the only factor the counting of Subject IV should be the most accurate for he had practice as subject or experimenter throughout the investigation That he is less accurate than Subject III who had practice during only one half of the investigation is due to the previous practice in sound experiments and greater sensitivity of Subject III. This subject had carried on extensive investigations with sound stimuli whereas Subject IV had carried on very few. Moreover actual test showed that Subject IV was less sensitive to differences in sounds. This previous practice and keener sensitivity enabled Subject III to reach his maximal efficiency in the preliminary experiments and he shows no decrease in the number of errors during the series. The improvement of Subject IV is greater than that of Subject I and II although in the first series, after the practice of the preliminary experiments, he still shows as high a proportion of errors as they do. The explanation of the latter fact, we believe, is to be found in the greater sensitivity of these two subjects while the explanation of the former is to be found in the greater practice of Subject IV in this investigation.

THE INFLUENCE OF THE DIFFERENT NUMBERS OF SOUNDS

In both arrangements of the second group of our experiments the subject was required to count sounds which might be any number from 1 to 6. Three hundred judgments with every pair of weights were taken when these sounds were given on the standard and three hundred when they were given on the comparison weight. Every number of sounds having been given on the same comparison weight the same number of times, in both groups there are 50 judgments on each pair of weights with each one of these different numbers of sounds. We may ask, then, whether the counting of one sound had the same, or a different, influence as the counting of six sounds. One might suppose that the effort varied with the number of sounds to be counted and the variation would show itself in different degrees of accuracy in judging the weights. To answer the question we collected from our original data the different judgments given with these different numbers of sounds. The absolute frequencies thus obtained are given in Tables 25 to 32. These tables are constructed in the same manner as the tables in which are the absolute frequencies of the experiments taken in regular order. The first column contains the number of sounds that were given on each comparison weight. At the head of the columns the comparison weights are found, each column containing the frequencies of the different classes of judgments. In the last line of a table is given the sum of these judgments for the six series. These data may be treated by the Method of Constant Stimuli and the values of h and S obtained in these calculations are found in Tables 45 to 48. The first column of these tables contains the number of sounds given on the different comparison weights. The divided columns marked h, S, etc., contain these values for the different series when the sounds were given on the standard and on the comparison weights. These numbers do not seem to show any regularity but before we are certain of this we must calculate the coefficient of correlation of the value h and the number of sounds given. On the basis of our present information, that is, on the basis of inspection of the values, we may conclude that the number of sounds to be counted has no influence on the judgment of the weight.

From these numbers, however, we are permitted to draw an important conclusion. If we calculated the coefficient of divergence for the six series of each of the groups with the discrete sounds we would be able to say very definitely that the judgments on each comparison stimulus were or were not given consistently. Lacking that coefficient we have a reliable index of the fundamental regularity of the judgments in the average result of the series taken in regular order and the average result in the series for the different number of sounds. The averages in these two cases are given in the last line of Tables 37-44 and 45-48. In the columns marked "sounds on C S" (comparison stimulus) of Tables 45-48, we find the averages which must be compared with the averages for the distraction series in Tables 37-40. The columns marked "sounds on S S" (standard stimulus) contain the averages which must be compared with the averages for the distraction series in Tables 41-44. The averages in both these cases are practically identical and from this fact the conclusion may be drawn that a fundamental regularity in the judgments existed throughout a group of experiments. This conclusion is possible because of the fact that the γ of an average of relative frequencies is not necessarily equal to the average of the γ's of the relative frequencies. If these two results are approximately equal we conclude that the original relative frequencies must have been nearly identical. Such is the result in this investigation and we see that the order in which the judgments are taken does not influence the result. Series may be taken in the order in which they are made or the series may be composed of every other judgment or every sixth judgment as in this case and the results are practically the same. There exists then a fundamental regularity throughout a group of experiments.

CONCLUSION

We wish to show the bearing of the results in this investigation on the distinction between voluntary and involuntary attention. This distinction is made on the basis of introspective evidence. In the case of voluntary attention the custom has been to speak of an activity of the mind, whereas in the case of involuntary attention no such activity could be postulated. This distinction is mentioned by Kulpe[1] where he says "Another current distinction makes attention voluntary and involuntary according as the will does or does not participate in its direction upon particular contents." This participation of the will is apparently what Kulpe means in the statement (page 425) "We have a general judgment of the origin of attention, in its description as an internal activity, concentrated in various degrees upon the particular ideas in consciousness. For this description only applies to attention, as given to introspection, on the assumption that we are able to perceive the internal activity as such. And this would mean, in its turn, that we are conscious of an internal activity above and beyond the contents upon which it is concentrated, and above and beyond the concomitant sensations to which its functioning gives rise." It is the presence of this internal activity as evidenced by introspection that distinguishes voluntary from involuntary attention.

In more recent works we see a different trend of thought. Wundt[2] says "If we apprehend an impression which is given to us without our assistance, the attention seems in a sense to be compelled to turn to this impression, following this single motive. We can express this by saying we apprehend it passively. The feeling of activity always follows such an impression. If on the other hand we turn to an expected impression, then these feelings of strain and excitation clearly precede the impression. We are aware that our apperception is active. These have often been called processes of involuntary and voluntary attention. But these expressions are unsuitable since in reality volitional processes are present in both cases. They are, like impulsive and voluntary acts merely processes of different grades." A similar view is offered by Pillsbury;[3] "We must in a large measure abandon the old systems of classification. There can be very little meaning given to passive and active, sensorial and intellectual, voluntary and involuntary attention. Classified from the standpoint of the conditions, we have only the more objectively con-conditioned attention and the more subjectively conditioned—attention which depends upon the strength or size of the external object and attention which depends upon interest or the mental condition at the moment."

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[1] O. Külpe, Outlines of Psychology, page 427.

[2] Wilhelm Wundt, Introduction to Psychology, 1911, page 67 [Trans.—R. Pinter].

Whether or not the recent thought is more satisfactory we shall not attempt to say at the present time nor shall we attempt to refute or uphold the division of attention on the basis of introspection. We would, however, indicate the new method of approach to the problem that we followed in our experiments. The conditions for the two groups of experiments were arranged to produce states of mind which were introspectively different. In the first group the distraction was produced by a sensory stimulation without a concomitant mental activity otherwise induced. In the second group the work of counting the clicks was a prominent feature of the mental process involved. The question was, whether this introspective difference had the same or a different influence on the estimation of the weights. The only way to answer this question is to observe the relative frequencies of the different judgments. We may illustrate the differences existing in the following way. Suppose we have two urns, the colors of which are different and which contain unknown numbers of differently colored balls. We wish to determine whether the relative frequencies or probabilities in the drawing of balls of a certain color are the same or different for the two urns. For this purpose we draw a number of balls from each urn, replacing the ball when the color is recorded. Obviously the contents of the urns remain the same and represent a constant group of conditions. If in a large number of drawings the results obtained from the two urns are the same, our conclusion is that the contents of the two urns are the same. Now the two differently colored urns correspond to two mental states which are introspectively different. The drawing of the ball of a certain color corresponds to the passing of a certain judgment. The psychophysical nature of the subject and the objective experimental arrangement are conditions which doubtlessly do not have the same high constancy as the contents of the urns but which nevertheless show a sufficient degree of stability to make possible a statistical investigation.

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[3] W. B. Pillsbury, Attention, 1908, page 311.

The results of this statistical investigation are practically identical for the two groups of experiments. For this fact we offer the same explanation as in the case of the two urns. On the basis of like relative frequencies in the drawings of a certain colored ball we conclude that the contents of the urns were the same. Likewise, on the basis of similar outcomes for the judgments when we have a voluntary and involuntary direction of attention we conclude that the conditions must be the same. This conclusion, however, is based on the statistical results alone and we may ask a further question,—why is it that mental states which to introspection are so widely different, give like results? Our statistical investigation can give no answer, at present, to the question. But, the facts of introspection are likewise unable to offer any solution; these facts are simply that such differences do exist.

For an explanation it is necessary to go behind the facts obtained by introspection and this statistical investigation,[4] and for this purpose more experimentation and further analysis is absolutely necessary. At present there seem to be two hypotheses either one of which may be satisfactory. In the first place it may be that our introspection is not an introspection of attention itself but introspection of the conditions of attention. These conditioning factors may be different while the processes of attention are the same whether voluntarily or involuntarily initiated. Thus we have the same system or group of causes to explain the same result. On the other hand, introspection may give us the subjective aspect of two different processes which are quite diverse and very complex, but in which the differences succeed in counteracting each other in such a way as to neutralize themselves and bring about identical results. A decision for either of these hypotheses can at the present time be only tentative. Which of them will prove the more satisfactory may be determined by future experimentation and further analysis along the line and by the method followed in this investigation.

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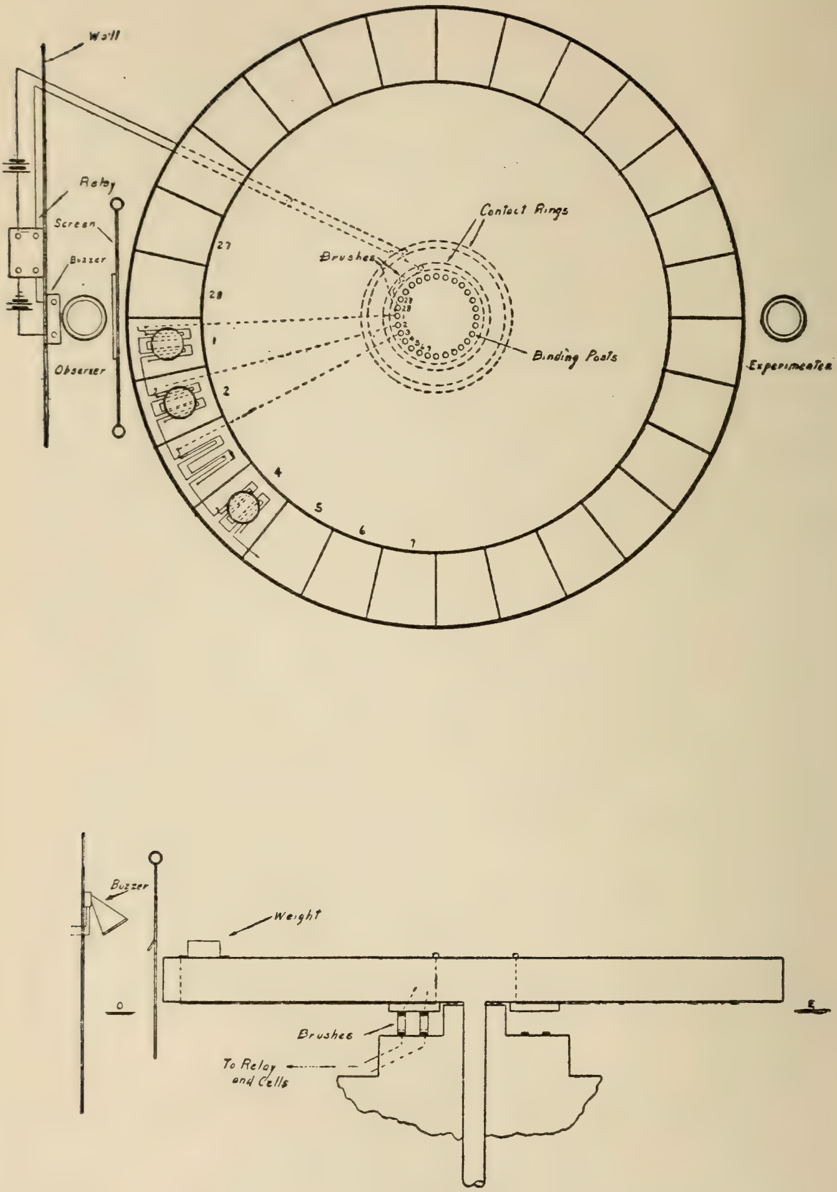
[4] See F. M. Urban, The Application of Statistical Methods to the Problems of Psychophysics, page 19—for a discussion of the different groups of causes underlying statistical results.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O
E
Brushes
To Relay
and Cells

FIGURE I

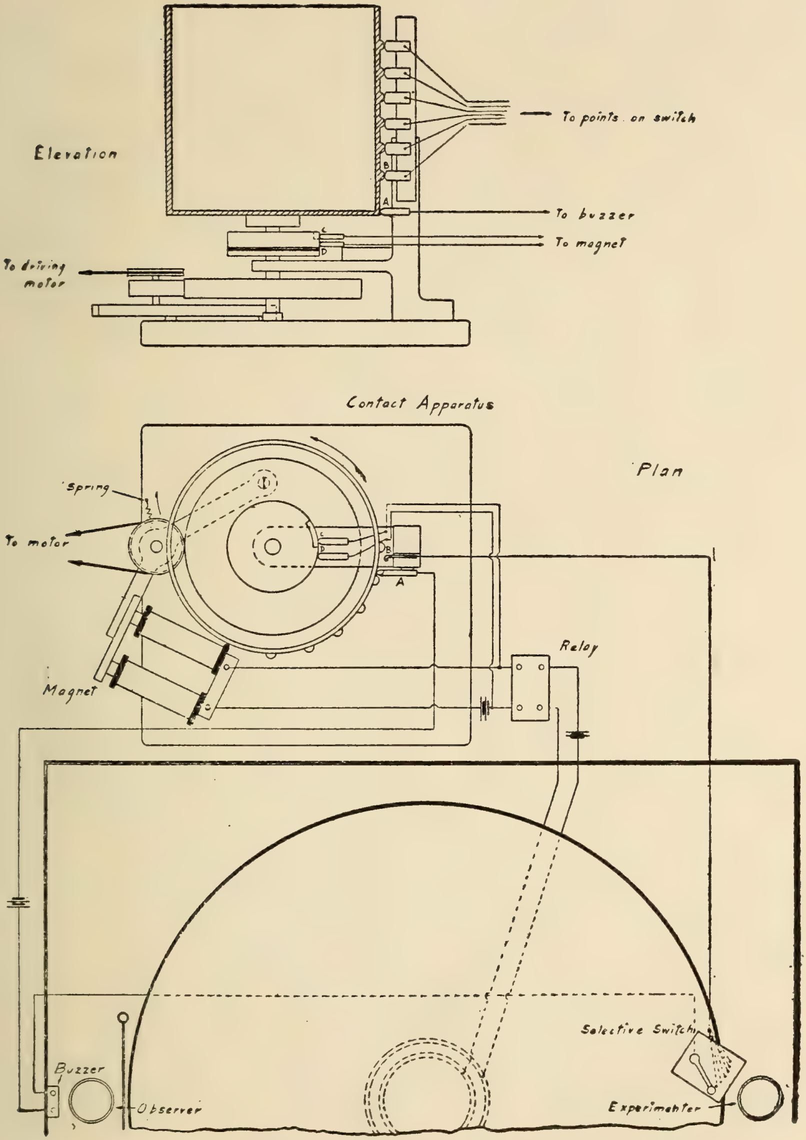
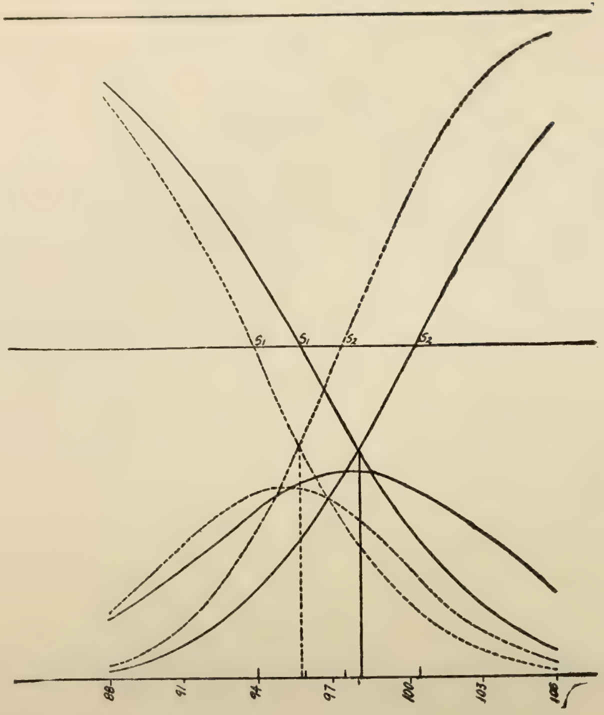

Elevation
To points on switch
B
A
To buzzer
C
D
To magnet
To driving motor
Contact Apparatus
Plan
Spring
To motor
C
D
B
A
Relay
Magnet
Selective Switch
Buzzer
Observer
Experimenter

FIGURE II

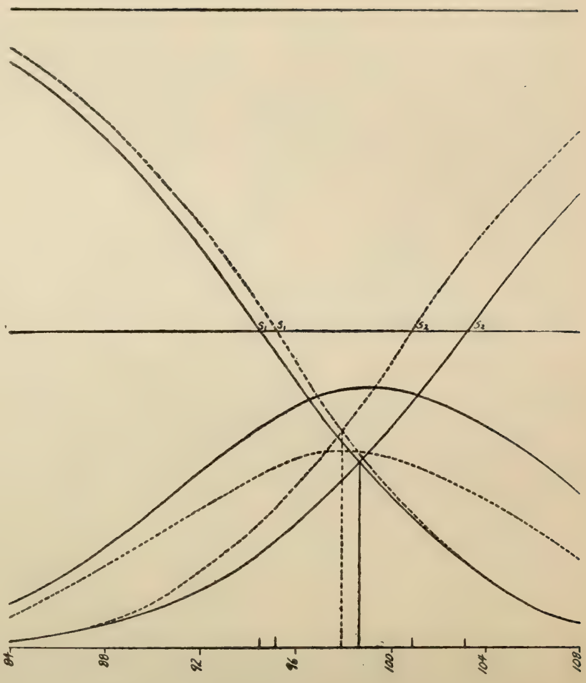
Subject I.
Series (1-10)

Chart I



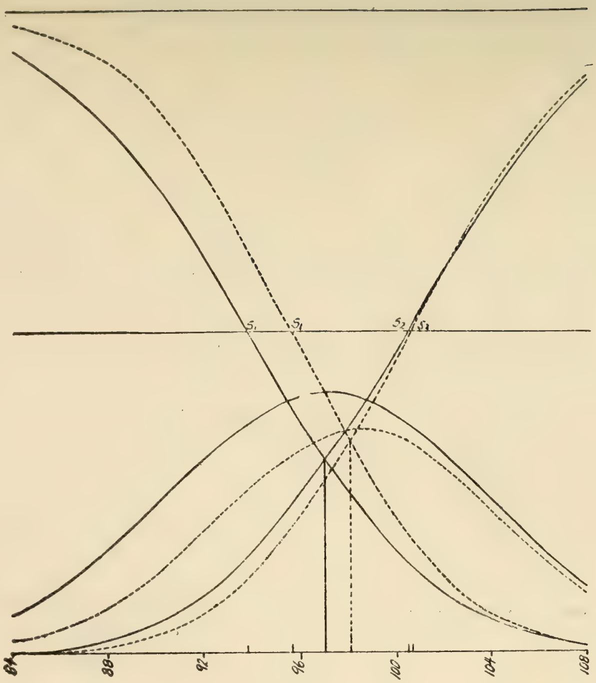
Subject II.
Series (1-10)

Chart 2.



Subject III
Series (1-10)

Chart 3.



Subject IV.
Series (1-10).

Chart 4

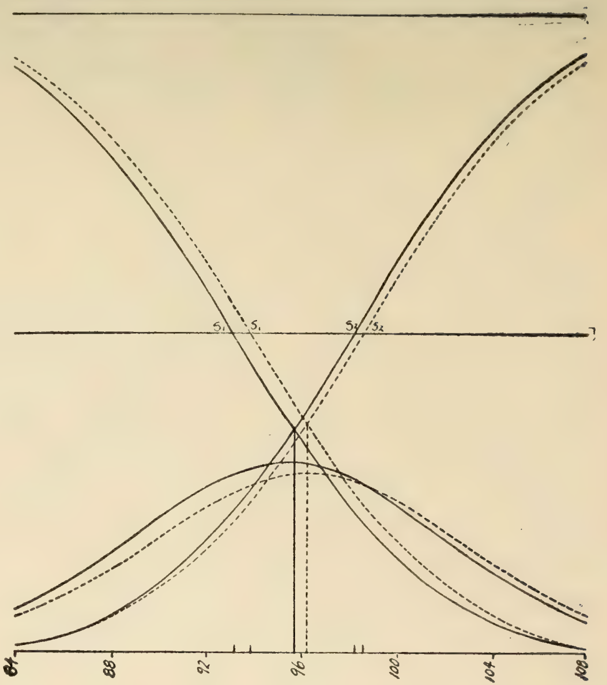


Chart 5.

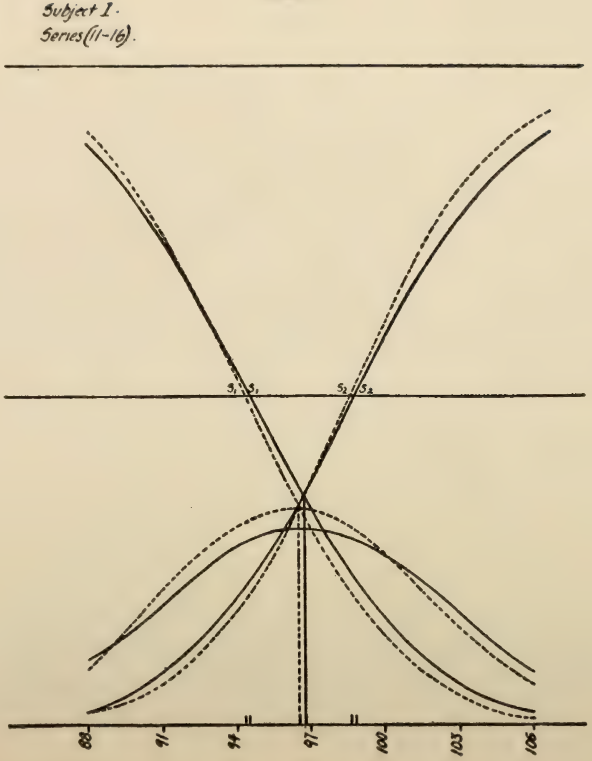


Chart 6.

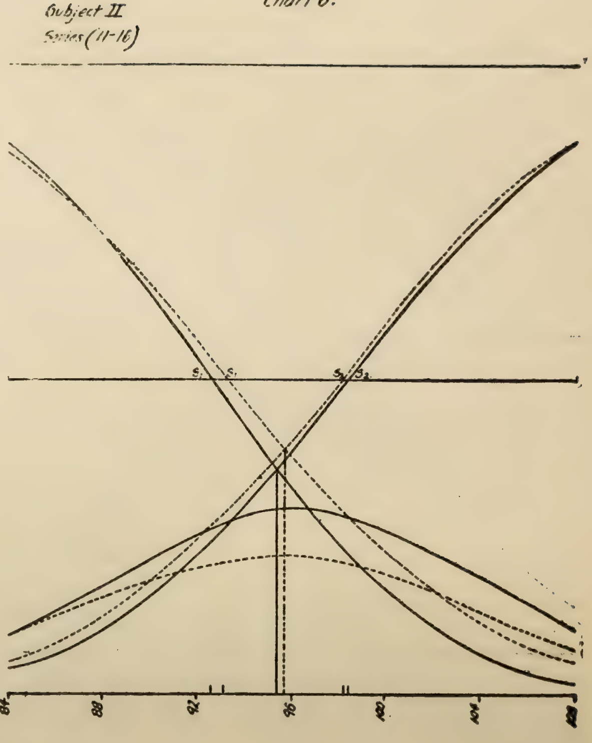


Chart 7.

Subject III
Series (11-16).

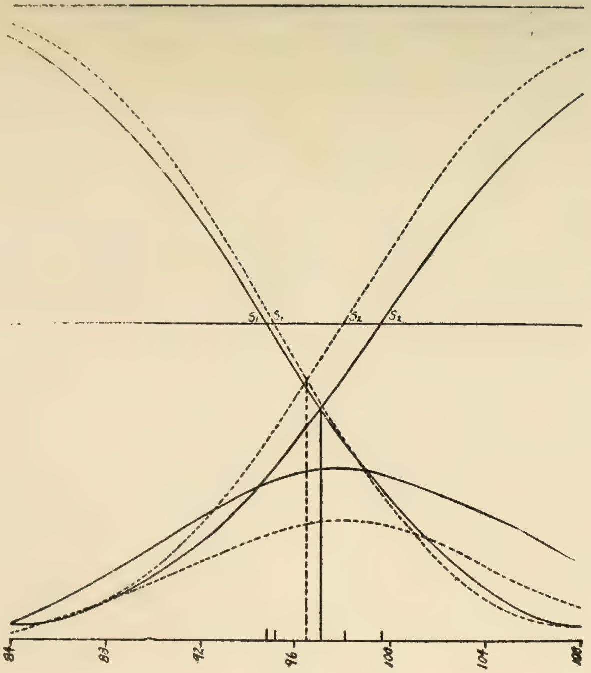


Chart 8

Subject IV.
Series (11-16).

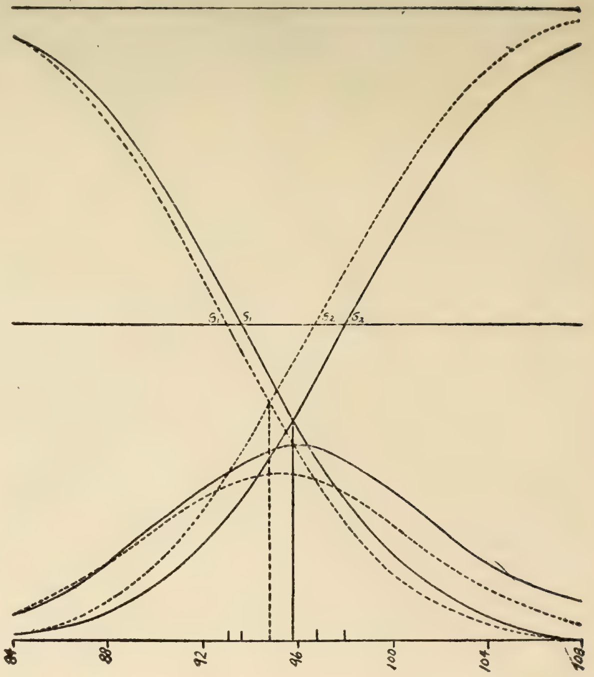


Chart 9.

Subject I.
Series (17-22)

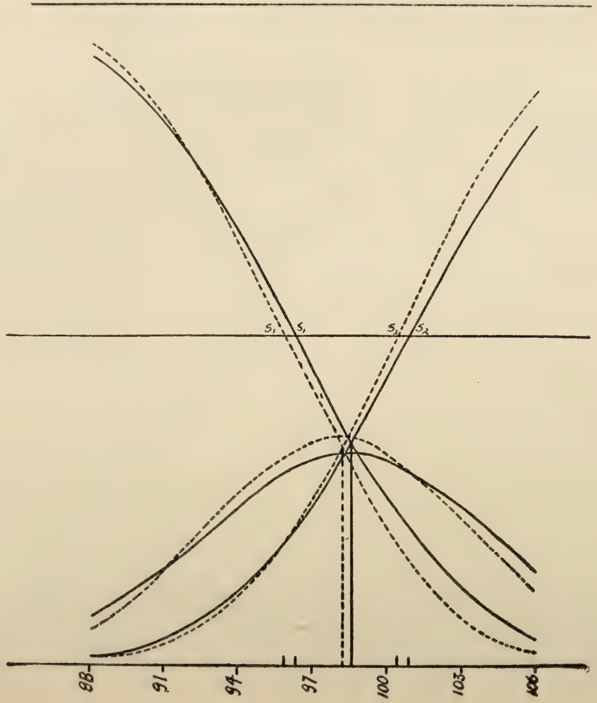


Chart 10

Subject II
Series (17-22)

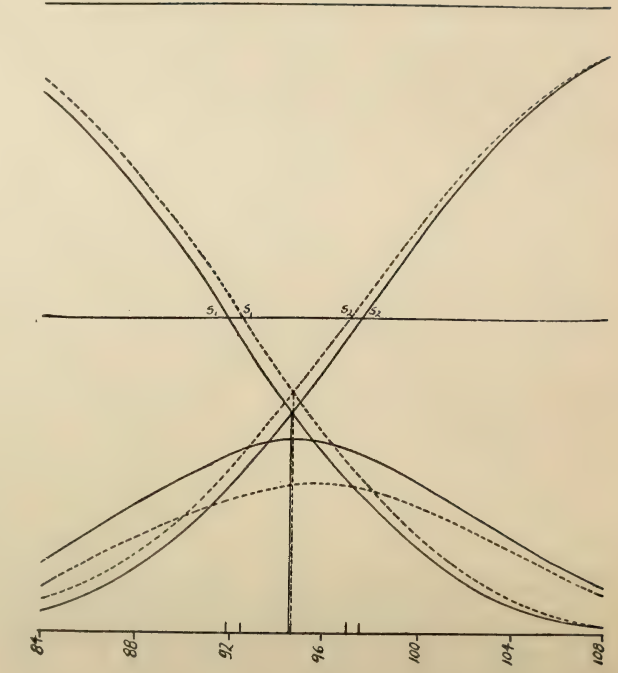


Chart 11.

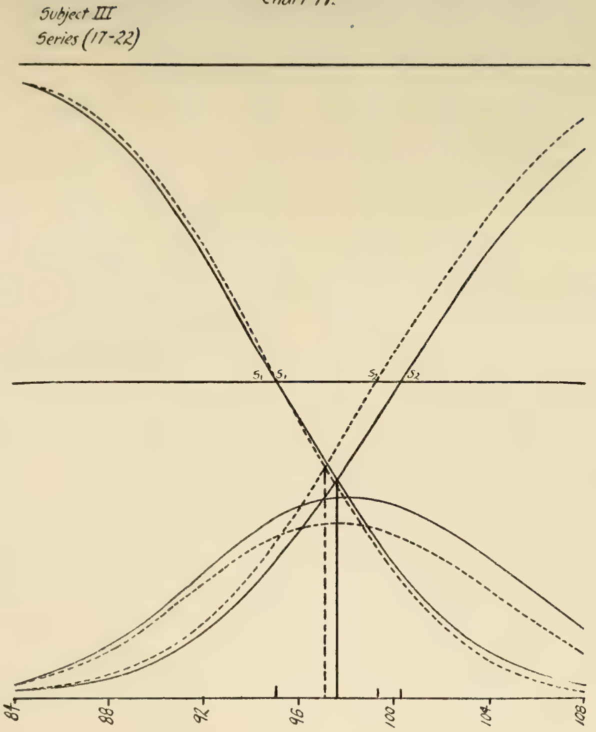
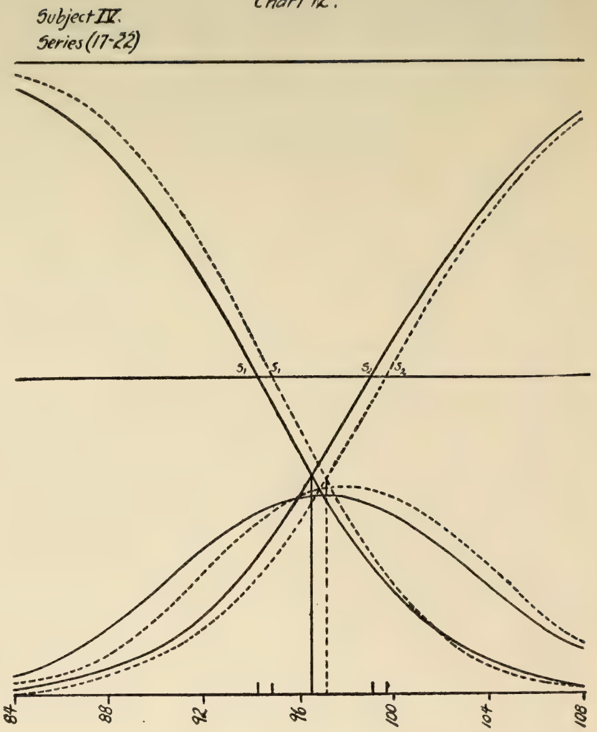


Chart 12.



APPENDIX

Table I—*Frequencies of Different Classes of Judgment—Without Sound—Subject I*

	88			91			94			97			100			103			106		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		6	44		12	38	4	16	30	18	15	17	23	13	14	34	13	3	41	5	4
2	1	5	44	2	7	41	3	12	35	16	14	20	29	9	12	32	9	9	42	6	2
3		3	47	2	8	40	7	7	36	12	21	17	21	17	12	31	10	9	47	3	
4	2	4	44		11	39	5	16	29	19	19	12	22	12	16	31	13	6	41	2	7
5	1	2	47	3	11	36	5	12	33	22	15	13	31	12	7	34	8	8	44	4	2
6		3	47	3	6	41	5	12	33	16	15	19	35	3	12	29	13	8	48	2	
7		5	45	1	7	42	5	12	33	11	24	15	27	17	6	41	5	4	39	7	4
8		3	47		5	45	9	17	24	13	18	19	26	19	5	35	9	6	37	10	3
9		4	46	2	9	39	3	14	33	9	20	21	23	16	11	34	10	6	36	11	3
10		1	49		9	41	4	15	31	7	21	22	21	16	13	28	17	5	36	14	
Σ	4	36	460	13	85	402	50	133	317	143	182	175	258	134	108	329	107	64	411	64	25

Table 2—*Frequencies of Different Classes of Judgment—Without Sound—Subject II*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		7	43		7	43	5	19	26	7	28	15	26	17	7	23	21	6	40	9	1
2	2	1	47		4	46	7	17	26	8	16	26	21	24	5	26	17	7	42	5	3
3		2	48	1	8	41	3	8	39	6	22	22	19	21	10	21	17	12	40	9	1
4		4	46	1	7	42	3	14	33	11	19	20	20	21	9	27	16	7	36	13	1
5	1	6	43	2	3	45	4	22	24	8	19	23	14	27	9	25	19	6	42	4	4
6	2	4	44	1	9	40	4	10	36	5	21	24	12	29	9	19	19	12	30	17	3
7		2	48		4	46	6	11	33	7	20	23	12	15	23	21	25	4	29	15	6
8	1	2	47	5	5	40	2	23	25	11	15	24	23	18	9	22	23	5	25	24	1
9	1	3	46	3	10	37	8	15	27	9	15	26	24	21	5	35	11	4	41	6	3
10		4	46	3	11	36	7	18	25	17	19	14	33	14	3	38	8	4	47	3	
Σ	7	35	458	16	68	416	49	157	294	89	194	217	204	207	89	257	176	67	372	105	23

Table 3—*Frequencies of the Different Classes of Judgment—Without Sound—Subject III*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1	9	40	2	13	35	4	19	27	10	27	13	18	23	9	22	25	3	40	9	1
2		2	48	1	9	40	2	28	20	5	27	18	19	28	3	27	20	3	44	4	2
3		5	45	2	7	41	1	22	27	9	21	20	23	24	3	26	22	2	41	8	1
4		7	43		9	41	4	15	31	6	24	20	30	19	1	23	26	1	43	5	2
5		4	46		7	43	1	16	33	9	24	17	28	21	1	33	15	2	45	4	1
6		4	46	1	2	47	2	10	38	9	16	25	28	20	2	26	13	11	44	3	3
7		1	49		4	46	9	14	27	19	6	25	27	12	11	39	8	3	47	2	1
8			50	1	1	48	5	14	31	16	14	20	28	10	12	38	8	4	48	2	
9	1	1	48	1		49	9	8	33	19	14	17	34	10	6	43	4	3	47	3	
10			50		5	45	7	19	24	18	16	16	36	11	3	43	5	2	49	1	
Σ	2	33	465	8	57	435	44	165	291	120	189	191	271	178	51	320	146	34	448	41	11

Table 4—*Frequencies of the Different Classes of Judgment—Without Sound—Subject IV*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		2	48	4	15	31	8	15	27	17	24	9	32	12	6	36	13	1	39	7	4
2	2	8	40	4	11	35	4	18	28	13	21	16	33	16	1	38	9	3	47	2	1
3	2	1	47	2	13	35	5	19	26	15	21	14	40	10		40	8	2	45	3	2
4		3	47	3	7	40	9	8	33	19	15	16	21	19	10	44	5	1	46	3	1
5		3	47		6	44	4	7	39	18	17	15	34	7	9	40	7	3	48	2	
6	1	2	47	3	7	40	4	8	38	23	17	10	35	11	4	48		2	49	1	
7	1	1	48		4	46	8	13	29	17	16	17	30	11	9	46	2	2	45	2	3
8		1	49	2	14	34	8	12	30	18	14	18	37	7	6	46	3	1	49	1	
9		4	46		6	44	11	13	26	17	20	13	39	10	1	43	3	4	47	1	2
10		2	48	4	13	33	9	7	34	18	11	21	38	7	5	36	11	3	50		
Σ	6	27	467	22	96	382	70	120	310	175	176	149	339	110	51	417	61	22	465	22	13

Table 5—*Frequencies of the Different Classes of Judgment—With Sound—Subject I*

	88			91			94			97			100			103			106		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		2	48	1	11	38	13	11	26	23	11	16	34	4	12	49	1		48	2	
2		1	49	2	9	39	14	13	23	17	16	17	37	9	4	44	2	4	48	2	
3	1	7	42	2	9	39	12	19	19	27	13	10	42	6	2	48	1	1	49	1	
4	2	3	45	1	5	44	11	19	20	23	15	12	33	9	8	45	4	1	47	2	1
5	1	9	40	4	4	42	16	14	20	20	19	11	36	8	6	43	6	1	46	3	1
6	2	6	42	4	7	39	14	16	20	23	10	17	37	5	8	41	8	1	45	4	1
7	3	7	40	4	10	36	9	16	25	22	17	11	44	4	2	43	5	2	50		
8	2	4	44	4	10	36	8	16	26	18	15	17	42	7	1	47	1	2	50		
9	1	6	43	5	11	34	4	18	28	21	15	14	36	8	6	46	1	3	49		1
10	1	7	42	3	8	39	8	17	25	28	12	10	36	10	4	43	7		48	2	
Σ	13	52	435	30	84	386	109	159	232	222	143	135	377	70	53	449	36	15	480	16	4

Table 6—*Frequencies of Different Classes of Judgment—With Sound—Subject II*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1	1	48	3	12	35	10	7	33	15	18	17	28	14	8	44	2	4	48	2	
2	2		48	1	4	45	8	13	29	12	18	20	25	17	8	30	13	7	45	3	2
3		1	49	1	3	46	6	11	33	18	9	23	23	14	13	29	16	5	44	5	1
4		3	47		8	42	2	19	29	14	11	25	27	12	11	32	12	6	39	8	3
5		1	49	2	6	42	5	13	32	7	14	29	23	17	10	34	14	2	42	5	3
6	1	5	44		8	42	5	11	34	12	17	21	21	21	8	30	9	11	43	5	2
7		2	48	1	8	41	6	9	35	3	16	31	27	13	10	28	16	6	39	8	3
8	1	2	47		5	45	5	8	37	13	15	22	21	19	10	27	13	10	34	7	9
9		3	47	2	9	39	4	13	33	12	14	24	23	18	9	33	10	7	33	10	7
10		2	48	5	9	36	3	17	30	11	17	22	24	20	6	36	11	3	46	3	1
Σ	5	20	475	15	72	413	54	121	325	117	149	234	242	165	93	323	116	61	413	56	31

Table 7—*Frequencies of Different Classes of Judgment—With Sound—Subject III*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1			50		11	39	6	13	31	8	15	27	19	23	8	34	13	3	41	8	1
2			50		2	48	1	15	34	5	23	22	19	22	9	38	11	1	45	5	
3		1	49		2	48	3	11	36	5	24	21	19	25	6	39	10	1	41	9	
4	1	5	44		3	47		12	38	2	13	35	21	23	6	26	16	8	39	9	2
5			50		1	49	1	10	39	3	18	29	23	21	6	30	14	6	44	5	1
6		2	48		1	49	4	5	41	6	11	33	30	15	5	29	13	8	40	10	
7		2	48		1	49	3	3	44	11	12	27	30	16	4	41	8	1	46	3	1
8			50		2	48		9	41	7	10	33	28	15	7	41	7	2	45	3	2
9	1		49		1	49	1	6	43	17	12	21	37	7	6	41	4	5	49	1	
10	1		49	2	8	40	6	8	36	19	20	11	30	9	11	43	4	3	46	2	2
Σ	3	10	487	2	32	466	25	92	383	83	158	259	256	176	68	362	100	38	436	55	9

Table 8—*Frequencies of the Different Classes of Judgments—With Sound—Subject IV*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1	4	45	5	8	37	4	12	34	19	16	15	23	16	11	36	10	4	41	5	4
2		7	43	6	17	27	10	19	21	19	22	9	25	16	9	38	9	3	40	10	
3		4	46	2	17	31	8	15	27	20	19	11	18	26	6	41	7	2	42	7	1
4	1	3	46	1	5	44	3	8	39	19	13	18	26	13	11	42	6	2	44	5	1
5	4	1	45	4	5	41	7	7	36	18	14	18	35	7	8	41	7	2	43	4	3
6			50	2	7	41	2	5	43	18	14	18	21	17	12	45	3	2	48	2	
7	2	1	47	2	1	47	4	15	31	18	8	24	37	8	5	45	3	2	47	3	
8	2	4	44	5	3	42	7	9	34	21	11	18	35	8	7	41	3	6	49		1
9		1	49	2	4	44	4	6	40	14	10	26	42	4	4	46	4		50		
10	1		49	2	2	46	9	8	33	27	8	15	43	4	3	38	8	4	46	3	1
Σ	11	25	464	31	69	400	58	104	338	193	135	172	305	119	76	413	60	27	450	39	11

TABLE 9—*Frequencies of the Different Classes of Judgment—Without Sound—Subject I*

	88			91			94			97			100			103			106		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11	1	8	41	8	8	34	12	14	24	12	18	20	35	11	4	31	17	2	45	4	1
12	3	5	42	5	14	31	7	14	29	22	16	12	27	18	5	33	15	2	43	6	1
13	3	2	45	1	11	38	12	9	29	18	16	16	32	12	6	41	5	4	47	2	1
14	1	2	47	3	7	40	9	10	31	21	11	18	35	10	5	36	9	5	46	4	
15		6	44	2	14	34	5	17	28	16	14	20	35	8	7	36	9	5	48	1	1
16		5	45		14	36	5	10	35	19	12	19	37	5	8	29	17	4	47	2	1
Σ	8	28	264	19	68	213	50	74	176	108	87	105	201	64	35	206	72	22	276	19	5

TABLE 10—*Frequencies of the Different Classes of Judgments—Without Sound—Subject II*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11	1	5	44	3	3	44	8	14	28	10	24	16	29	12	9	36	10	4	42	7	1
12	1	5	44	3	4	43	13	18	19	22	12	16	35	7	8	37	9	4	45	4	1
13		3	47	3	10	37	8	23	19	21	14	15	32	12	6	36	12	2	42	7	1
14		4	46	7	7	36	13	10	27	18	17	15	32	10	8	43	3	4	43	7	
15	1	8	41	4	8	38	17	5	28	27	12	11	32	14	4	38	9	3	43	6	1
16	2	5	43	9	8	33	10	16	24	16	16	18	33	9	8	32	9	9	43	3	4
Σ	5	30	265	29	40	231	69	86	145	114	95	91	193	64	43	222	52	26	258	34	8

TABLE 11—*Frequencies of the Different Classes of Judgments—Without Sound—Subject III*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11		2	48	1	2	47	12	9	29	12	14	24	28	12	10	28	16	6	46	2	2
12			50	4	1	45	6	12	32	11	11	28	32	11	7	34	11	5	46	4	
13		1	49			50	9	12	29	10	11	29	26	12	12	31	16	3	42	6	2
14	1	3	46		3	47	1	10	39	18	15	17	36	8	6	32	12	6	48	1	1
15			50	4	5	41	3	15	32	14	18	18	29	15	6	32	9	9	41	7	2
16		6	44	2	5	43	6	7	37	24	12	14	32	14	4	32	14	4	44	5	1
Σ	1	12	287	11	16	273	37	65	198	89	81	130	183	72	45	189	78	33	267	25	8

TABLE 12—*Frequencies of the Different Classes of Judgment—Without Sound—Subject IV*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11	1	1	48		13	37	6	9	35	25	19	6	35	11	4	46	4		47	2	1
12	1	2	47	2	12	36	7	6	37	26	16	8	36	11	3	48	1	1	50		
13			50	1	9	40	4	18	28	19	18	13	37	12	1	46	4		48	2	
14	1		49	4	6	40	8	10	32	17	18	15	31	13	6	46	4		45	2	3
15			50	1	4	45	5	7	38	16	14	20	28	11	11	37	10	3	46	4	
16		1	49	4	5	41	4	10	36	19	17	14	20	16	14	36	12	2	43	5	2
Σ	3	4	293	12	49	239	34	60	206	122	102	76	187	74	39	259	35	6	279	15	6

TABLE 13—*Frequencies of the Different Classes of Judgment—With Sound—Subject I*

	88			91			94			97			100			103			106		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11	1	2	47	3	8	39	7	13	30	12	15	23	28	13	9	43	6	1	47	2	1
12		6	44	5	12	33	12	8	30	14	22	14	30	14	6	45	5		50		
13		1	49	1	12	37	8	21	21	23	18	9	36	8	6	41	9		47	3	
14	2	3	45	2	11	37	11	13	26	14	23	13	29	10	11	38	10	2	43	5	2
15	1	5	44	3	7	40	7	14	29	15	17	18	36	13	1	37	12	1	48		2
16	1	6	43	6	5	39	9	15	26	10	30	10	41	4	5	42	8		47	3	
Σ	5	23	272	20	55	225	54	84	162	88	125	87	200	62	38	246	50	4	282	13	5

Table 14—*Frequencies of the Different Classes of Judgments—With Sounds—Subject II*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11	2	2	46	4	4	42	10	15	25	14	13	23	28	18	4	40	7	3	45	3	2
12	2	4	44	6	10	34	13	12	25	20	13	17	33	9	8	38	8	4	43	4	3
13		3	47	4	11	35	18	10	22	16	13	21	33	8	9	36	5	9	44	3	3
14		4	46	8	7	35	12	8	30	22	6	22	32	11	7	34	5	11	44		6
15	2	5	43	7	10	33	14	7	29	21	10	19	36	7	7	38	5	7	41	4	5
16	3	7	40	7	7	36	9	6	35	16	14	20	29	11	10	40	5	5	45	3	2
Σ	9	25	266	36	49	215	76	58	166	109	69	122	191	64	45	226	35	39	262	17	21

Table 15—*Frequencies of the Different Classes of Judgments—With Sound—Subject III*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11	2	1	47	2	1	47	5	9	36	21	8	21	35	6	9	44	3	3	49	1	
12			50	3	1	46	12	2	36	15	11	24	24	13	13	44	4	2	44	4	2
13		1	49	1	2	47	7	13	30	13	11	26	31	9	10	32	13	5	47	2	1
14	1	1	48	3		47	8	1	41	17	8	25	41	7	2	44	2	4	45	3	2
15	1	2	47	4	2	44	6	3	41	19	14	17	35	9	6	42	5	3	46	3	1
16	4	3	43	2	2	46	9	8	33	14	14	22	43	4	3	40	8	2	45	4	1
Σ	8	8	284	15	8	277	47	36	217	99	66	135	209	48	43	246	35	19	276	17	7

Table 16—*Frequencies of the Different Classes of Judgment—With Sound—Subject IV*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
11		2	48	5	8	37	7	16	27	25	16	9	40	8	2	43	7		46	2	2
12		2	48	3	5	42	8	7	35	27	12	11	40	9	1	45	4	1	45	4	1
13		1	49	1	11	38	13	11	26	27	15	8	44	3	3	49		1	50		
14		3	47	4	5	41	5	12	33	26	13	11	26	16	8	45	5		48	2	
15	2		48	3	5	42	6	7	37	24	11	15	32	9	9	46	1	3	50		
16	1		49	2	7	41	11	9	30	20	20	10	32	10	8	48	2		50		
Σ	3	8	289	18	41	241	50	62	188	149	87	64	214	55	31	276	19	5	289	8	3

Table 17—*Frequencies of the Different Classes of Judgment—Without Sounds—Subject I*

	88			91			94			97			100			103			106		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17		5	45	6	7	37	8	11	31	13	25	12	21	17	12	34	12	4	47	3	
18	3	5	42	1	11	38	8	5	37	17	20	13	27	16	7	28	18	4	46	3	1
19		3	47	1	5	44	6	11	33	8	15	27	16	17	17	24	15	11	42	7	1
20		4	46	1	9	40	6	8	36	13	16	21	25	14	11	27	13	10	42	6	2
21		4	46	1	8	41	3	11	36	13	13	24	31	12	7	29	13	8	46	3	1
22		2	48		5	45	3	9	38	9	16	25	22	16	12	20	16	14	44	4	2
Σ	3	23	274	10	45	245	34	55	211	73	105	122	142	92	66	162	87	51	267	26	7

Table 18—*Frequencies of the Different Classes of Judgment—Without Sound—Subject II*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17	1	5	44	9	6	35	15	15	20	21	14	15	38	7	5	40	8	2	49	1	
18	1	7	42	7	9	34	12	13	25	22	14	14	33	12	5	40	8	2	45	3	2
19		5	45	3	7	40	12	14	24	20	12	18	39	8	3	41	7	2	50		
20		5	45	3	6	41	16	11	23	20	17	13	34	12	4	39	6	5	43	7	
21	1	6	43	6	12	32	9	19	22	19	17	14	39	9	2	36	9	5	48	2	
22	1	7	42	3	16	31	6	16	28	20	16	14	33	13	4	35	12	3	43	6	1
Σ	4	35	261	31	56	213	70	88	142	122	90	88	216	61	23	231	50	19	278	19	3

TABLE 19—*Frequencies of the Different Classes of Judgment—Without Sound—Subject III*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17		3	47	1	3	46	5	8	37	9	16	25	33	11	6	37	12	1	49		1
18	1	5	44	2	1	47	5	12	33	16	8	26	31	12	7	32	13	5	46	4	
19	1		49	3	4	43	6	11	33	12	13	25	24	16	10	36	11	3	46	3	1
20		2	48		1	49	1	9	40	14	16	20	25	16	9	27	17	6	43	7	
21		1	49		1	49	5	10	35	7	26	17	30	19	1	31	15	4	44	4	2
22			50		3	47	7	10	33	13	14	23	23	19	8	22	24	4	47	3	
Σ	2	11	287	6	13	281	29	60	211	71	93	136	166	93	41	185	92	23	275	21	4

TABLE 20—*Frequencies of the Different Classes of Judgment—Without Sound—Subject IV*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17		1	49	2	4	44	9	8	33	17	16	17	33	14	3	39	7	4	49	1	
18			50	3	5	42	2	14	34	11	20	19	27	18	5	40	8	2	44	5	1
19		2	48	1	6	43	2	18	30	18	17	15	30	15	5	37	11	2	47	2	1
20	1	1	48	1	4	45	4	10	36	18	16	16	28	12	10	34	11	5	47	2	1
21	1	1	48	2	7	41	1	12	37	18	23	9	30	14	6	38	9	3	45	5	
22	1	4	45	3	8	39	4	13	33	13	18	19	30	15	5	39	10	1	47	2	1
Σ	3	9	288	12	34	254	22	75	203	95	110	95	178	88	34	227	56	17	279	17	4

TABLE 21—*Frequencies of the Different Classes of Judgments—With Sound—Subject I*

	88			91			94			97			100			103			106		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17	1	5	44	4	9	37	5	13	32	15	21	14	30	12	8	42	6	2	48	2	
18		6	44	3	12	35	9	9	32	19	18	13	14	16	20	41	6	3	47	2	1
19		2	48	1	5	44	2	10	38	12	23	15	18	23	9	36	12	2	45	4	1
20		6	44	1	6	43	5	10	35	9	18	23	22	22	6	31	14	5	37	11	2
21	1	3	46		8	42	5	10	35	9	20	21	24	14	12	39	9	2	43	5	2
22	1		49	1	6	43	1	9	40	4	19	27	26	13	11	32	12	6	36	13	1
Σ	3	22	275	10	46	244	27	61	212	68	119	113	134	100	66	221	59	20	256	37	7

TABLE 22—*Frequencies of the Different Classes of Judgments—With Sound—Subject II*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17		3	47	5	9	36	19	11	20	28	11	11	39	11		36	11	3	46	2	2
18		1	49	1	4	45	12	11	27	29	11	10	36	9	5	44	3	3	46	2	2
19		2	48	8	7	35	22	9	19	27	13	10	41		9	43	4	3	45	4	1
20	3	2	45	2	4	44	5	14	31	21	11	18	35	6	9	39	8	3	45	4	1
21	1	7	42	5	14	31	11	14	25	21	18	11	34	13	3	38	7	5	41	6	3
22		6	44	2	7	41	9	16	25	17	19	14	30	15	5	36	9	5	46	3	1
Σ	4	21	275	23	45	232	78	75	147	143	83	74	215	54	31	236	42	22	269	21	10

TABLE 23—*Frequencies of the Different Classes of Judgments—With Sound—Subject III*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17			50	2	5	43	13	8	29	20	7	23	43	2	5	43	4	3	49	1	
18	1		49	1	4	45	7	8	35	22	13	15	27	14	9	44	4	2	48	2	
19			50	1	4	45	11	13	26	13	10	27	25	13	12	37	9	4	45	3	2
20		2	48		2	48	6	9	35	6	16	28	25	18	7	33	11	6	46	3	1
21		1	49		1	49	2	9	39	8	15	27	35	13	2	29	16	5	42	8	
22			50	1	3	46		8	42	8	17	25	26	22	2	31	13	6	45	5	
Σ	1	3	296	5	19	276	39	55	206	77	78	145	181	82	37	217	57	26	275	22	3

TABLE 24—*Frequencies of the Different Classes of Judgment—With Sound—Subject IV*

	84			88			92			96			100			104			108		
Series	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
17		3	47	4	8	38	3	7	40	17	22	11	35	12	3	44	4	2	40	10	
18			50		5	45	4	9	37	15	19	16	31	13	6	43	7		44	6	
19			50	2	1	47	2	9	39	18	18	14	23	20	7	40	7	3	46	3	1
20	1	1	48		1	49	1	5	44	11	21	18	27	15	8	34	8	8	48	2	
21	1	1	48	1	5	44	1	7	42	9	15	26	24	15	11	41	6	3	43	3	4
22			50		2	48	3	8	39	13	22	15	22	21	7	40	8	2	46	4	
Σ	2	5	293	7	22	271	14	45	241	83	117	100	162	96	42	242	40	18	267	28	5

TABLE 25—*Frequencies of the Different Classes of Judgments with Selected Sounds on Comparison Weight—Subject I*

No. of	88			91			94			97			100			103			106		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1	3	46	1	15	34	9	13	28	14	25	11	36	9	5	41	9		46	3	1
2	1	3	46	1	12	37	10	21	19	11	24	15	38	9	3	33	16	1	49	1	
3		5	45	3	6	41	10	14	26	24	14	12	28	14	8	45	5		44	3	3
4	1	1	48	6	5	39	8	10	32	14	21	15	34	9	7	43	7		48	2	
5	2	6	42	5	12	33	7	19	24	13	20	17	30	12	8	40	9	1	48	2	
6		5	45	4	5	41	10	7	33	12	21	17	34	9	7	44	4	2	47	2	1
Σ	5	23	272	20	55	225	54	84	162	88	125	87	200	62	38	246	50	4	282	13	5

TABLE 26—*Frequences of the Different Classes of Judgment with Selected Sounds on Comparison Weight—Subject II*

No. of	84			88			92			96			100			104			108		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1	2	47	8	6	36	10	10	30	23	8	19	34	12	4	33	5	12	44	1	5
2	1	2	47	5	6	39	16	9	25	11	14	25	29	13	8	41	3	6	43	5	2
3	2	5	43	8	9	33	14	7	29	18	12	20	30	13	7	35	7	8	42	2	6
4	3	8	39	3	7	40	13	12	25	20	8	22	32	9	9	39	8	3	46	3	1
5	1	5	44	8	11	31	12	10	28	21	15	14	38	4	8	39	5	6	44	2	4
6	1	3	46	4	10	36	11	10	29	16	12	22	28	13	9	39	7	4	43	4	3
Σ	9	25	266	36	49	215	76	58	166	109	69	122	191	64	45	226	35	39	262	17	21

TABLE 27—*Frequencies of the Different Classes of Judgment with Selected Sounds on Comparison Weight—Subject III*

No. of	84			88			92			96			100			104			108		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		2	48	2		48	6	3	41	15	15	20	35	9	6	42	3	5	45	3	2
2	2		48	1	1	48	11	6	33	16	8	26	34	11	5	41	9		42	6	2
3	2		48	4	3	43	9	6	35	14	13	23	35	9	6	41	7	2	47	2	1
4	2	3	45	2	1	47	7	4	39	14	15	21	33	7	10	37	8	5	44	4	2
5	1	3	46	3	2	45	9	7	34	20	8	22	32	8	10	40	6	4	48	2	
6	1		49	3	1	46	5	10	35	20	7	23	40	4	6	45	2	3	50		
Σ	8	8	284	15	8	277	47	36	217	99	66	135	209	48	43	246	35	19	276	17	7

TABLE 28—*Frequencies of the Different Classes of Judgments with Selected Sounds on Comparison Weight—Subject IV*

No. of	84			88			92			96			100			104			108		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		1	49	3	6	41	11	7	32	16	19	15	34	11	5	46	3	1	49	1	
2		1	49	1	6	43	3	16	31	25	14	11	36	9	5	46	3	1	48	2	
3	1	2	47	4	3	43	5	9	36	21	18	11	38	8	4	48	2		46	3	1
4	1	1	48	2	11	37	11	11	28	28	13	9	33	13	4	45	5		49	1	
5	1	1	48	5	6	39	10	8	32	31	12	7	36	5	9	46	3	1	49	1	
6		2	48	3	9	38	10	11	29	28	11	11	37	9	4	45	3	2	48		2
Σ	3	8	289	18	41	241	50	62	188	149	87	64	214	55	31	276	19	5	289	8	3

Table 29—*Frequencies of Different Classes of Judgments with Selected Sounds on Standard Weight—Subject I*

No. of	88			91			94			97			100			103			106		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		5	45	2	10	38	3	13	34	13	18	19	25	15	10	37	12	1	43	5	2
2		1	49	1	5	44	3	11	36	12	15	23	23	17	10	36	11	3	43	7	
3	1	6	43	2	4	44	8	8	34	8	26	16	20	20	10	40	8	2	40	10	
4	1	4	45	2	6	42	4	13	33	12	20	18	24	14	12	33	12	5	42	6	2
5		2	48		10	40	4	12	34	13	21	16	20	14	16	38	7	5	43	6	1
6	1	4	45	3	11	36	5	4	41	10	19	21	22	20	8	37	9	4	45	3	2
Σ	3	22	275	10	46	244	27	61	212	68	119	113	134	100	66	221	59	20	256	37	7

Table 30—*Frequencies of the Different Classes of Judgment with Selected Sounds on Standard Weight—Subject II*

No. of	84			88			92			96			100			104			108		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1	3	46	5	8	37	16	15	19	23	19	8	40	7	3	45	5		48	2	
2	3	2	45	6	10	34	19	9	22	26	9	15	36	9	5	35	9	6	47	3	
3		6	44	6	8	36	11	12	27	23	11	16	41	6	3	42	5	3	45	3	2
4		3	47	3	9	38	12	12	26	31	13	6	37	8	5	40	8	2	47	3	
5		3	47	2	6	42	11	14	25	18	15	17	29	11	10	36	6	8	38	8	4
6		4	46	1	4	45	9	13	28	22	16	12	32	13	5	38	9	3	44	2	4
Σ	4	21	275	23	45	232	78	75	147	143	83	74	215	54	31	236	42	22	269	21	10

Table 31—*Frequencies of the Different Classes of Judgment with Selected Sounds on the Standard Weight—Subject III*

No. of	84			88			92			96			100			104			108		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1	1		49		6	44	6	7	37	17	7	26	33	11	6	35	9	6	46	4	
2		2	48		4	46	8	11	31	11	16	23	33	11	6	34	10	6	48	1	1
3		1	49	1	1	48	7	10	33	11	18	21	28	16	6	38	7	5	45	4	1
4			50	1	1	48	8	5	37	14	10	26	30	11	9	38	10	2	43	7	
5			50	3	4	43	6	11	33	11	12	27	30	17	3	37	10	3	46	3	1
6			50		3	47	4	11	35	13	15	22	27	16	7	35	11	4	47	3	
Σ	1	3	296	5	19	276	39	55	206	77	78	145	181	82	37	217	57	26	275	22	3

Table 32—*Frequencies of the Different Classes of Judgment with Selected Sounds on Standard Weight—Subject IV*

No. of	84			88			92			96			100			104			108		
S.	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l	h	e	l
1		1	49		5	45	2	7	41	15	21	14	35	10	5	39	8	3	46	2	2
2		1	49		4	46	4	8	38	17	21	12	25	18	7	38	9	3	46	3	1
3			50	2	4	44	1	9	40	12	22	16	25	24	1	40	6	4	46	4	
4		2	48		5	45	4	8	38	13	21	16	28	13	9	41	6	3	43	7	
5	1	1	48	3	3	44	1	8	41	9	16	25	24	15	11	40	6	4	44	6	
6	1		49	2	1	47	2	5	43	17	16	17	25	16	9	44	5	1	42	6	2
Σ	2	5	293	7	22	271	14	45	241	83	117	100	162	96	42	242	40	18	267	28	5

TABLE 33—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject I*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
1	0.1092	0.1307	95.39	95.09	0.1222	0.1656	100.36	97.46	4.97	2.37	98.02	96.42
2	0.1136	0.1468	96.20	94.64	0.1276	0.1566	100.21	97.73	4.01	3.09	98.32	96.23
3	0.1199	0.1598	96.31	93.14	0.1354	0.1841	100.48	96.66	4.17	3.52	98.52	95.03
4	0.0949	0.1464	95.57	94.30	0.1068	0.1452	100.48	97.74	4.91	3.44	98.17	96.01
5	0.1200	0.1336	95.20	93.51	0.1267	0.1340	99.27	97.59	4.07	4.08	97.29	95.55
6	0.1223	0.1272	96.22	94.00	0.1351	0.1255	99.51	97.63	3.29	3.63	97.95	95.80
7	0.1252	0.1380	95.46	93.27	0.1389	0.1500	100.30	97.10	4.84	3.83	98.01	95.26
8	0.1288	0.1432	95.47	94.06	0.1012	0.1731	100.44	97.16	4.97	3.10	97.66	95.76
9	0.1185	0.1262	95.98	94.03	0.1246	0.1676	101.48	97.65	5.50	3.62	98.80	96.10
10	0.1339	0.1516	96.21	93.62	0.1176	0.1554	101.63	97.53	5.42	3.91	98.74	95.60
A	0.1186	0.1404	95.80	93.98	0.1236	0.1558	100.42	97.42	4.62	3.46	98.15	95.78

TABLE 34—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject II*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
1	0.0940	0.1014	93.10	93.53	0.0906	0.1132	102.38	98.05	9.28	4.52	97.66	95.91
2	0.0965	0.1022	94.87	94.77	0.0809	0.0900	102.03	100.46	7.16	5.69	98.14	97.43
3	0.0943	0.1142	95.92	95.62	0.0997	0.0938	103.42	100.60	7.50	4.98	99.78	97.87
4	0.0982	0.0922	94.60	95.02	0.0895	0.0934	102.91	101.05	8.31	6.03	98.56	98.05
5	0.0826	0.1042	94.09	95.40	0.0904	0.1040	103.01	101.17	8.92	5.77	98.75	98.28
6	0.0797	0.0850	95.41	94.92	0.0674	0.0942	106.81	101.27	11.40	6.35	100.63	98.26
7	0.0888	0.0972	96.81	95.90	0.0658	0.0955	106.38	102.00	9.57	6.10	100.88	98.92
8	0.0973	0.0814	93.99	96.65	0.0569	0.0740	105.73	102.90	11.74	6.25	98.32	99.63
9	0.0900	0.0898	93.76	95.42	0.0903	0.0820	100.68	102.24	6.92	6.82	97.23	98.68
10	0.1047	0.1046	92.22	93.56	0.1094	0.1018	98.44	100.00	6.22	6.44	95.40	96.74
A	0.0926	0.0972	94.48	95.08	0.0841	0.0942	103.18	101.01	8.70	5.90	98.54	97.98

TABLE 35—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject III*

Series	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
	Sound		Sound		Sound		Sound		Sound		Sound	
1	0.0864	0.1028	91.81	94.33	0.0842	0.1014	103.15	101.55	11.34	7.22	97.41	97.92
2	0.1032	0.1472	92.74	95.09	0.1192	0.1549	102.35	101.44	9.61	6.35	97.90	98.35
3	0.1096	0.1542	93.07	94.84	0.0991	0.1252	102.20	101.60	9.13	6.76	97.40	97.87
4	0.1002	0.0964	93.30	96.30	0.1025	0.0931	101.78	102.92	8.48	6.62	97.59	99.55
5	0.1188	0.1353	93.60	96.49	0.1292	0.1411	100.80	102.02	7.20	5.53	97.35	99.31
6	0.0956	0.1196	96.23	96.93	0.1110	0.1008	101.45	101.54	5.22	4.61	99.03	99.04
7	0.1155	0.1434	94.94	95.83	0.1033	0.1356	98.49	99.56	3.55	3.73	96.62	97.64
8	0.1169	0.1410	95.08	96.76	0.1242	0.1398	99.17	101.18	4.09	3.42	97.19	98.46
9	0.1323	0.1425	94.37	95.92	0.1168	0.1288	97.71	97.96	3.34	2.04	95.94	96.89
10	0.1337	0.1074	93.07	94.20	0.1346	0.1136	97.64	98.30	4.57	4.10	95.36	96.31
A	0.1112	0.1290	93.82	95.67	0.1124	0.1234	100.47	100.71	6.65	5.04	97.18	98.13

TABLE 36—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject IV*

Series	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
	Sound		Sound		Sound		Sound		Sound		Sound	
1	0.0871	0.0857	91.78	93.88	0.0820	0.0863	99.42	100.01	7.64	6.13	95.49	96.96
2	0.0916	0.0843	91.76	90.44	0.1036	0.0762	98.68	99.21	6.92	8.77	95.43	94.60
3	0.0974	0.1006	92.55	91.70	0.1054	0.0940	98.23	99.72	5.68	8.02	95.50	95.57
4	0.1110	0.1132	93.82	94.76	0.1052	0.1106	98.67	99.21	4.85	4.45	96.18	96.96
5	0.1205	0.0932	94.54	94.18	0.1292	0.0886	98.41	97.94	3.87	3.76	96.54	96.01
6	0.1298	0.1194	93.21	95.07	0.1354	0.1326	96.85	99.04	3.64	3.97	95.07	97.16
7	0.1078	0.1268	94.41	94.39	0.1059	0.1188	98.16	97.51	3.75	3.12	96.30	95.90
8	0.1135	0.0978	92.88	94.04	0.1404	0.1071	97.17	97.18	4.29	3.14	95.26	95.68
9	0.1054	0.1400	93.15	95.16	0.1125	0.1623	97.14	97.34	3.99	2.18	95.21	96.33
10	0.1044	0.1277	93.41	94.30	0.0980	0.1047	98.02	97.00	4.61	2.70	95.64	95.52
A	0.1068	0.1089	93.15	93.79	0.1118	0.1081	98.08	98.42	4.92	4.62	95.66	96.07

TABLE 37—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject I*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
11	0.1219	0.1420	93.80	95.46	0.1072	0.1515	99.06	99.02	5.26	3.56	96.26	97.30
12	0.1258	0.1324	93.54	94.12	0.1053	0.1399	99.37	98.26	5.83	4.14	96.20	96.25
13	0.1334	0.1679	94.75	93.82	0.1310	0.1548	98.18	98.17	3.43	4.35	96.45	95.91
14	0.1422	0.1237	95.31	94.55	0.1327	0.1211	98.57	99.30	3.26	4.75	96.88	96.90
15	0.1179	0.1366	94.64	94.73	0.1575	0.1472	99.05	98.74	4.41	4.01	97.16	96.81
16	0.1289	0.1514	95.23	93.84	0.1301	0.1430	99.23	98.18	4.00	4.34	97.24	95.95
A	0.1284	0.1423	94.54	94.42	0.1273	0.1429	98.91	98.61	4.36	4.19	96.70	96.52

TABLE 38—*Values of h and S., Interval of Uncertainty, and Point of Subjective Equality—Subject II*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
11	0.1005	0.1024	93.52	93.61	0.0940	0.0942	99.94	98.67	6.42	5.06	96.62	96.03
12	0.0970	0.0826	92.62	92.47	0.0935	0.0823	97.69	97.84	5.07	5.37	95.11	95.15
13	0.1050	0.0766	92.05	93.34	0.0888	0.0800	98.80	97.87	6.75	4.53	95.14	95.66
14	0.0974	0.0710	92.80	94.42	0.0867	0.0760	97.98	98.28	5.18	3.86	95.24	96.41
15	0.0990	0.0722	91.71	93.07	0.0819	0.0778	97.18	97.55	5.47	4.48	94.18	95.39
16	0.0696	0.0801	92.60	93.45	0.0752	0.0864	98.77	98.19	6.17	4.74	95.80	95.91
A	0.0948	0.0808	92.55	93.39	0.0867	0.0828	98.39	98.06	5.84	4.67	95.35	95.76

TABLE 39—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject III*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
11	0.1022	0.1218	95.24	94.99	0.0888	0.1216	100.06	97.23	4.82	2.24	97.48	96.11
12	0.1148	0.1130	95.32	95.77	0.1016	0.0975	99.43	98.73	4.11	2.96	97.25	97.14
13	0.1056	0.1160	95.34	95.44	0.0872	0.1090	100.67	99.66	5.33	4.22	97.75	97.48
14	0.1128	0.1234	94.91	95.83	0.1067	0.1122	98.88	97.49	3.97	1.66	96.84	96.62
15	0.0906	0.1220	94.27	94.64	0.0898	0.1093	100.42	97.82	6.15	3.18	97.33	96.14
16	0.1087	0.1096	93.66	93.82	0.0920	0.0958	98.95	97.55	5.29	3.73	96.08	95.56
A	0.1058	0.1176	94.79	95.08	0.0944	0.1076	99.74	98.08	4.94	3.00	97.12	96.51

TABLE 40—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject IV*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
11	0.1234	0.1098	92.66	92.23	0.1190	0.1071	97.00	96.72	4.34	4.49	94.79	94.45
12	0.1333	0.1331	92.43	93.33	0.1400	0.1094	96.40	96.66	3.97	3.33	94.46	94.83
13	0.1495	0.1413	92.42	92.01	0.1417	0.1698	97.24	95.26	4.82	3.25	94.76	93.78
14	0.1024	0.1216	93.91	93.34	0.1073	0.1150	97.98	97.51	4.07	4.17	95.99	95.37
15	0.1273	0.1199	95.32	94.34	0.1157	0.1169	99.45	97.02	4.13	2.68	97.29	95.66
16	0.1075	0.1500	94.69	92.76	0.0921	0.1260	100.10	96.98	5.41	4.22	97.18	94.69
A	0.1239	0.1293	93.57	93.00	0.1193	0.1240	98.03	96.69	4.46	3.69	95.74	94.80

TABLE 41—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject I*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
17	0.1240	0.1365	94.94	94.62	0.1173	0.1524	99.92	98.75	4.98	4.13	97.36	96.80
18	0.1279	0.1101	94.82	95.26	0.1108	0.1345	99.96	99.51	5.14	4.25	97.20	97.59
19	0.1187	0.1655	97.37	95.93	0.1254	0.1634	102.10	100.72	4.73	4.79	99.80	98.31
20	0.1171	0.1304	96.44	95.91	0.1233	0.1233	100.96	101.62	4.52	5.71	98.76	98.66
21	0.1295	0.1360	96.27	96.07	0.1397	0.1418	100.50	100.25	4.23	4.18	98.46	98.20
22	0.1261	0.1516	97.65	97.09	0.1366	0.1287	101.90	101.73	4.25	4.64	99.86	99.22
A	0.1239	0.1384	96.25	95.81	0.1255	0.1407	100.89	100.43	4.64	4.62	98.57	98.13

TABLE 42—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject II*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	—	+	—	+	—	+	—	+	—	+	—	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
17	0.1026	0.0955	91.50	91.89	0.0956	0.0796	96.18	95.82	4.68	3.93	93.76	93.67
18	0.0843	0.1152	91.46	93.42	0.0902	0.1051	97.30	96.43	5.84	3.01	94.48	94.86
19	0.1135	0.0974	92.51	91.76	0.1114	0.0809	96.96	94.59	4.45	2.83	94.71	93.04
20	0.1031	0.1064	92.48	93.96	0.0842	0.0976	97.54	98.01	5.06	4.05	94.76	95.90
21	0.0894	0.0800	91.26	91.17	0.0995	0.0844	97.51	98.21	6.25	7.04	94.54	94.78
22	0.0920	0.0986	91.48	92.69	0.0944	0.1019	98.98	98.88	7.50	6.19	95.28	95.84
A	0.0975	0.0988	91.78	92.48	0.0959	0.0916	97.41	96.99	5.63	4.51	94.59	94.68

Table 43—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject III*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
17	0.1252	0.1186	94.98	94.09	0.1336	0.1250	99.33	96.44	4.35	2.35	97.22	95.29
18	0.1006	0.1347	94.88	94.47	0.1021	0.1197	99.55	97.98	4.67	3.51	97.23	96.12
19	0.1160	0.0996	95.09	94.97	0.1021	0.1000	99.89	99.50	4.80	4.53	97.34	97.24
20	0.1221	0.1180	95.76	95.83	0.1008	0.1164	101.30	100.70	5.54	4.87	98.27	98.25
21	0.1230	0.1384	94.94	95.98	0.1098	0.1087	100.72	100.94	5.78	4.96	97.67	98.16
22	0.1263	0.1310	95.17	95.94	0.0870	0.1172	101.47	101.00	6.30	5.04	97.74	98.33
A	0.1189	0.1234	95.14	95.21	0.1059	0.1145	100.38	99.43	5.24	4.21	97.58	97.23

Table 44—*Values of h and S., Interval of Uncertainty and Point of Subjective Equality—Subject IV*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
Series	Sound		Sound		Sound		Sound		Sound		Sound	
17	0.1293	0.1277	96.16	93.14	0.1166	0.0978	98.12	98.73	1.96	5.59	96.04	95.56
18	0.1217	0.1527	94.08	94.34	0.1132	0.1186	99.86	99.06	5.78	4.72	96.86	96.40
19	0.1228	0.1362	93.57	94.96	0.1236	0.1178	99.24	99.51	5.67	4.55	96.42	97.07
20	0.1150	0.1204	94.93	96.05	0.1083	0.1168	99.46	100.11	4.53	4.06	97.13	98.05
21	0.1262	0.1070	93.60	96.31	0.1079	0.1150	99.11	100.42	5.51	4.11	96.14	98.44
22	0.1096	0.1586	93.33	94.96	0.1126	0.1303	99.03	100.08	5.70	5.12	96.22	97.27
A	0.1208	0.1338	94.28	94.96	0.1137	0.1160	99.14	99.65	4.86	4.69	96.47	97.13

Table 45—*Values of h and S., etc., with Selected Sounds on S and C Weights—Subject I*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
No. of	Sounds		Sounds		Sounds		Sounds		Sounds		Sounds	
S	on S S	on C S	on S S	on C S	on S S	on C S	on S S	on C S	on S S	on C S	on S S	on C S
1	0.1293	0.1425	95.39	94.01	0.1452	0.1507	100.23	98.66	4.84	4.65	97.95	96.40
2	0.1642	0.1603	96.42	93.71	0.1520	0.1427	100.55	99.06	4.13	5.35	98.41	96.23
3	0.1381	0.1240	95.34	94.77	0.1263	0.1318	100.61	98.32	5.27	3.55	97.86	96.60
4	0.1260	0.1602	95.85	95.05	0.1294	0.1464	100.66	98.20	4.81	3.15	98.28	96.55
5	0.1318	0.1208	96.08	93.81	0.1490	0.1354	100.48	98.78	4.40	4.97	98.41	96.44
6	0.1264	0.1450	95.73	95.17	0.1371	0.1514	100.20	98.37	4.47	3.20	98.06	96.80
A	0.1360	0.1421	95.80	94.42	0.1398	0.1431	100.45	98.56	4.65	4.14	98.16	96.50

TABLE 46—*Values of h and S., etc., with Selected Sounds on S and C Weights—Subject II*

No. of S	h_1 Sounds on S S	h_1 Sounds on C S	S_1 Sounds on S S	S_1 Sounds on C S	h_2 Sounds on S S	h_2 Sounds on C S	S_2 Sounds on S S	S_2 Sounds on C S	Interval of Uncertainty Sounds on S S	Interval of Uncertainty Sounds on C S	Point of Subjective Equality Sounds on S S	Point of Subjective Equality Sounds on C S
1	0.1358	0.0747	91.03	94.25	0.1117	0.1007	95.79	97.88	4.76	3.63	93.18	96.34
2	0.0886	0.0914	91.99	94.06	0.0811	0.0872	96.34	98.46	4.35	4.40	94.07	96.21
3	0.0952	0.0686	92.36	93.31	0.0960	0.0737	96.53	98.43	4.17	5.12	94.46	95.96
4	0.1229	0.0848	92.00	92.71	0.0999	0.0911	96.34	97.58	4.34	4.87	93.94	95.24
5	0.0831	0.0752	94.25	92.34	0.0771	0.0872	99.69	97.09	5.44	4.75	96.87	94.89
6	0.0987	0.0878	93.52	93.76	0.0974	0.0910	98.36	98.86	4.84	5.10	95.92	96.36
A	0.1040	0.0804	92.52	93.40	0.0939	0.0885	97.18	98.05	4.65	4.64	94.74	95.83

TABLE 47—*Values of h and S., etc., with Selected Sounds on S and C Weights—Subject III*

No. of S	h_1 Sounds on S S	h_1 Sounds on C S	S_1 Sounds on S S	S_1 Sounds on C S	h_2 Sounds on S S	h_2 Sounds on C S	S_2 Sounds on S S	S_2 Sounds on C S	Interval of Uncertainty Sounds on S S	Interval of Uncertainty Sounds on C S	Point of Subjective Equality Sounds on S S	Point of Subjective Equality Sounds on C S
1	0.1177	0.1215	95.59	95.68	0.1027	0.1164	98.89	98.52	3.30	2.84	97.13	97.07
2	0.1129	0.1180	94.94	95.31	0.1106	0.0931	99.21	98.38	4.27	3.07	97.05	96.66
3	0.1242	0.1220	95.12	94.62	0.1135	0.1042	99.68	97.83	4.56	3.21	97.30	96.10
4	0.1476	0.1074	95.74	95.24	0.1028	0.0974	99.42	99.03	3.68	3.79	97.25	97.05
5	0.1200	0.1074	94.75	94.88	0.1094	0.1093	99.40	97.81	4.65	2.93	96.96	96.36
6	0.1318	0.1349	95.21	95.00	0.1211	0.1333	99.92	96.88	4.71	1.88	97.47	95.94
A	0.1257	0.1185	95.22	95.12	0.1100	0.1090	99.42	98.08	4.20	2.95	97.19	96.53

TABLE 48—*Values of h and S., etc., with Selected Sounds on S and C Weights—Subject IV*

No. of S	h_1 Sounds on S S	h_1 Sounds on C S	S_1 Sounds on S S	S_1 Sounds on C S	h_2 Sounds on S S	h_2 Sounds on C S	S_2 Sounds on S S	S_2 Sounds on C S	Interval of Uncertainty Sounds on S S	Interval of Uncertainty Sounds on C S	Point of Subjective Equality Sounds on S S	Point of Subjective Equality Sounds on C S
1	0.1265	0.1375	94.91	93.42	0.1266	0.1268	99.04	97.18	4.13	3.76	96.97	95.22
2	0.1325	0.1459	94.64	93.24	0.1158	0.1423	99.56	97.34	4.92	4.10	96.93	95.27
3	0.1343	0.1273	94.64	93.51	0.1252	0.1181	99.85	97.10	5.21	3.59	97.15	95.24
4	0.1266	0.1362	94.74	92.10	0.1145	0.1212	99.72	96.50	4.98	4.40	97.11	94.17
5	0.1167	0.1238	95.84	92.98	0.1081	0.1204	100.16	95.78	4.32	2.80	97.92	94.36
6	0.1235	0.1090	95.55	92.76	0.1060	0.1196	99.52	96.32	3.97	3.56	97.39	94.62
A	0.1267	0.1300	95.05	93.00	0.1160	0.1247	99.64	96.70	4.59	3.70	97.24	94.81

Table 49—*Average of Series* 1—10 *for each Subject*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
Sub	Sound		Sound		Sound		Sound		Sound		Sound	
I	0.1186	0.1404	95.80	93.98	0.1236	0.1558	100.42	97.42	4.62	3.46	98.15	95.78
II	0.0926	0.0972	94.48	95.08	0.0841	0.0942	103.18	101.01	8.70	5.90	98.54	97.98
III	0.1112	0.1290	93.82	95.67	0.1124	0.1234	100.47	100.71	6.65	5.04	97.18	98.13
IV	0.1068	0.1089	93.15	93.79	0.1118	0.1081	98.08	98.42	4.92	4.62	95.66	96.07
A	0.1073	0.1189	94.31	94.63	0.1080	0.1204	10054	99.39	6.22	4.76	97.38	96.99

Table 50—*Average of Series* 11—16 *for each Subject*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
Sub	Sound		Sound		Sound		Sound		Sound		Sound	
I	0.1284	0.1423	94.54	94.42	0.1273	0.1429	98.91	98.61	4.36	4.19	96.70	96.52
II	0.0948	0.0808	92.55	93.39	0.0867	0.0828	98.39	98.06	5.84	4.67	95.35	95.76
III	0.1058	0.1176	94.79	95.08	0.0944	0.1076	99.74	98.08	4.94	3.00	97.12	96.51
IV	0.1239	0.1293	93.57	93.00	0.1193	0.1240	98.03	96.69	4.46	3.69	95.74	94.80
A	0.1132	0.1175	93.86	93.97	0.1069	0.1143	98.77	97.86	4.90	3.89	96.23	95.90

Table 51—*Average of Series* 17—22 *for each Subject*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
	−	+	−	+	−	+	−	+	−	+	−	+
Sub	Sound		Sound		Sound		Sound		Sound		Sound	
I	0.1239	0.1384	96.25	95.81	0.1255	0.1407	100.89	100.43	4.64	4.62	98.57	98.13
II	0.0975	0.0988	91.78	92.48	0.0959	0.0916	97.41	96.99	5.63	4.51	94.59	94.68
III	0.1189	0.1234	95.14	95.21	0.1059	0.1145	100.38	99.43	5.24	4.21	97.58	97.23
IV	0.1208	0.1338	94.28	94.96	0.1137	0.1160	99.14	99.65	4.86	4.69	96.47	97.13
A	0.1153	0.1236	94.36	94.62	0.1102	0.1157	99.46	99.12	5.09	4.51	96.80	96.79

Table 52—*Comparison of Results of Years* 1912 *and* 1913—*Subject I*

	h_1		S_1		h_2		S_2		Interval of Uncertainty		Point of Subjective Equality	
Series	1912	1913	1912	1913	1912	1913	1912	1913	1912	1913	1912	1913
1	0.1162	0.1186	94.11	95.80	0.1206	0.1236	99.69	100.42	5.58	4.62	96.99	98.15
2	0.1034	0.1284	94.58	94.54	0.1125	0.1273	99.51	98.91	4.93	4.36	97.15	96.70
3	0.1103	0.1239	95.10	96.25	0.1287	0.1255	99.62	100.89	4.52	4.64	97.55	98.57
A	0.1100	0.1236	94.60	95.53	0.1206	0.1255	99.61	100.07	5.01	4.54	97.23	97.80

TABLE 53—*Errors made on Numbers of Different Subjects*

	Numbers Given						Total
Sub	1	2	3	4	5	6	
III	1	2	2	4	9	4	22
IV		1	6	36	76	86	205
I	5	6	19	76	124	102	332
II	7	4	9	85	120	167	392
Σ	13	13	36	201	329	359	951

TABLE 54—*Errors of the Different Subjects on Numbers in Series with Discrete Sounds*

	SERIES											
Sub	1	2	3	4	5	6	7	8	9	10	11	12
I	52	40	29	26	28	31	28	19	19	22	23	15
II	41	41	44	27	49	37	35	48	31	23	8	8
III	3	3	1	1		2	1	1		9	1	
IV	47	17	16	25	14	25	16	23	6	4	5	7
Σ	143	101	90	79	91	95	80	91	56	58	37	30

Vol. XVII
No. 4

PSYCHOLOGICAL REVIEW PUBLICATIONS

December, 1914
Whole No. 75

THE
Psychological Monographs

EDITED BY

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YALE PSYCHOLOGICAL STUDIES

NEW SERIES—VOLUME II, NO. 1

Edited By

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PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.);
LEIPZIG (Koenigstr., 37); PARIS (16 rue de Condé)

PRINCETON
UNIVERSITY
PRESS

EDITOR'S PREFACE

This issue of the Yale Psychological Studies comprises two investigations made by candidates for the degree of Doctor of Philosophy under the direction of Charles H. Judd, former Director of the Yale Psychological Laboratory, now Director of the School of Education, University of Chicago. The present editor is responsible, however, for the editing of the manuscripts in preparation for the press.

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EXPERIMENTAL ANALYSIS OF THE WRITING MOVEMENT[1]

FRANK NUGENT FREEMAN, PH.D.

Assistant Professor of Educational Psychology, University of Chicago.

The experiment here reported consisted in an analysis of the handwriting movement of adults and of children by measurements of the size, speed and pressure of writing. By an interpretation of the results some of the psychological factors in the writing process and in its development have been determined. Writing is characterized by a decided rhythm which is more pronounced in the developed than in the undeveloped movement. In this as well as in other ways the movements by which the successive parts of a complex letter or series of letters are produced are united in such a way as to indicate that writing psychologically is not a succession of separate acts, but is an organized process in which individual elements have their place in the larger units of letters, words or groups of words. The component elements in the process come to be treated not as individual strokes or movements but rather as stages in the progress of the organized whole. The writing of children stands, however, in contrast to more developed writing in that it is less rhythmical, less organized and less automatized. That the child gives more continuous attention to the details of the writing movement is inferred from the fact that the movement is more uniform in speed throughout the different parts of the letter.

I. THE PROBLEM

The writing movement may be analyzed in two ways. In the first place the number of component movements—finger, hand, arm, etc.—may be determined and their nature investigated as in the experiments of Judd (**11**) and Obici (**17**) ; and in the second place the characteristics of the total resultant movement may be investigated. The present report deals with an investigation of the second type. The characteristics of the writing movement which are here studied are the speed, pressure and size of writing, and their relations to each other. Taken together they furnish a good objective characterization of the writing movement. Such a study was made in order to determine the stages and course of development of the writing movement. A characterization of writing has often been sought in terms of the external form. The latest use of external form as a basis for study has been made by Thorndike (**19**) and Ayers (**3**) in their construction of scales for the measurement of the excellence of handwriting.[2] The present investigation, however, aimed to determine the differences in writing at different stages of its development in terms of the motor coördination by which it is produced. In this report a method for such a study is described and some comparative results of its application to the writing of adults and of children are given in order, as just stated, to ascertain the course of development of the movement. The application of the method to the study of the development of the writing movement is by no means complete and the writer plans to continue this study, and to present further results in the near future.

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[1] A condensation of a thesis submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy in Yale University, in May, 1908.

II. HISTORICAL SKETCH

A report of the experiments may be prefaced by a brief discussion of previous investigations in the experimental analysis of the writing movement by methods of the same type as the one here used. The discussion may be divided into two parts, concerning first, the investigation of the speed of writing, and second, of the pressure and speed together.

A study of the speed of finger and hand movements of different degrees of complexity, including the writing movement, was made by Jack (**10**). The movements had three degrees of complexity: first, simple flexion and extension of a single finger; second, a straight movement with the pen involving several fingers; third, an ordinary writing movement. The distinction between the second and third types is not altogether clear except, perhaps, that the latter included curves as well as straight lines. The method was to require the subject to trace the lines on a piece of smoked glass carried on one prong of a vibrating tuning fork. The problem was to discover the modifications in the different types of movement with differences in age, education and manual skill. The result in general was that old age, lack of education or of manual skill, as well as certain pathological conditions, affected unfavorably the complex movements to a far greater extent than the simpler ones. The simple flexion and extension movements were made with practically the same rapidity by all classes of subjects and also with the right and left hands.

---

[2] The writer has also prepared for publication in the near future a scale to be included in *The Teaching of Handwriting,* Houghton Mifflin Co., Boston.

The speed of writing movements was more extensively studied by Binet and Courtier (**4**). These authors used the Edison pen. This is an apparatus by means of which a needle is driven downward by a rotating eccentric at a rate of 1,000 strokes per minute. Thus when one writes with the Edison pen a dotted line is traced, and the distance between the dotes indicates the speed of the movement. There are two defects in this method. In the first place the pen weighs about a quarter of a pound and the conditions are therefore not normal. In the second place, there is no record of the time of pauses. The results were as follows. The speed was greater when the writing movements were large and when they were accustomed movements than in the contrary cases. The direction in which the most rapid movements were made was the diagonal from lower left to upper right. The middle of the stroke was more rapid than the beginning and the end, connected writing was faster than disconnected, and intentional increase in speed corresponded to a decrease in size. A change in direction was accomplished by a reduction in speed, the amount of reduction being proportional to the angular change in direction. The more conditions which had to be satisfied by the movement, the slower it was. There was in general found to be a uniformity in speed variations in similar movements.

The relative speed of a writing movement in different directions was studied incidentally in connection with a general investigation of voluntary movement by Woodworth (**21**). He found that the most rapid movement was a side to side forearm movement and that the vertical finger movement was much slower.

The relative speed of finger, wrist and arm movements in different directions was investigated by McAllister (**12**). He used two methods. In the first the movements of the finger were recorded by the breaking of an electric circuit at the beginning and the end of the movement. The subject moved with his fingers the upper end of a vertical rod pivoted in the middle, the lower end of which released the contact. By the second method the speed of the movement was recorded by an electric spark produced by an induction coil, which passed from the pen to a metal sheet underneath the paper. The paper was covered with a coat of lampblack and the sparks were recorded as dots on the line of the writing. The defects of this method are first, that the possibility of a shock introduces a distraction; and second, that the time of pauses, as in Binet and Courtier's experiment, could not be recorded. McAllister found that movements in the direction from lower left to upper right and the reverse direction were the fastest, and that the arm movement was somewhat faster than the finger-wrist movement. He also found that the direction of a movement was unintentionally diverted in the direction of the greatest ease.

A piece of apparatus based on the same principle and called a graphometer was constructed by MacMillan (**13**). This consisted essentially of a small disc containing a groove along one of the radii and pivoted in the center so that the groove could be set in any direction. At each end of the groove were contacts and an electric circuit was made when the stylus touched either of these contacts. MacMillan, like Woodworth, found that the most rapid movements were made by a side to side arm and wrist motion.

The pressure of writing was first analyzed by Goldscheider (**8**). The method was to record the pressure by means of a tambour upon which the writing surface was supported. Goldscheider found that there is a characteristic form of pressure curve for the different letters and strokes and concluded that the sensations of pressure form an important means to the recognition and control of the writing movement.

Gross (**9**) analyzed the writing movement as a means to clinical diagnosis. For comparison he studied a number of normal individuals. The apparatus used was Kraepelin's writing balance (*Schriftwage*). This consists essentially of a small table resting on the short arm of a lever, the other end of which records upon a kymograph. By adding a time line on the drum the total speed and average speed per letter may be measured. Gross confirmed Goldscheider's result that the pressure curves from a given individual for a given letter were uniform and that the corresponding pressure curves from different individuals presented great similarity. The pressure in general was higher on the down stroke than on the up stroke. Exceptions occurred, however, at the beginning and end of a letter or group of letters. The pressure showed a more abrupt release at the end than was the on-set at the beginning. There was a gradual increase during the course of writing of a series of letters or figures. The speed and size of characters had an intimate relation and tended to vary together. In other words, the total time of a character tended to remain constant and the writing manifested a rhythmical character. In the course of the writing the speed, pressure and size increased together. This was attributed to an increasing nervous excitation (*Anregung*). The same fact appeared at the beginning of writing and was attributed to the initial impulse (*Antrieb*). The author found greater variation in the length of the pauses between the strokes than in the duration of the letters themselves. He confirmed the result found by Jack, that there is greater difference between individuals in the complex than in the simpler strokes and that in pathological cases the differences due to education and skill tend to disappear.

The same method and apparatus was used by Diehl (**5**), who carried on his investigation for the purpose of testing and completing Gross's determination of the normal type. Diehl found in general a similar relation between the three elements, speed, pressure and size, as was found by Gross. An increase in the general nervous excitation or activity (*Anregung*) produced an increase in the speed, pressure and size. An increase in the mental difficulty attending writing, such as writing characters in the reverse order, produced a decrease in the speed and size and an increase in pressure. Voluntary increase in speed produced a decrease in size and an increase in pressure. The changes in the last two instances are the same except for speed, which in the latter case is subject to voluntary control. An analysis of the two cases shows that an increase in effort produces a decrease in size and an increase in pressure.

This effect of effort upon writing has been determined incidentally in connection with other investigations. Urbantschitsch (**20**) found that stimulation by a high musical tone produced a crowding of the letters together and a decrease in size, accompanied by a feeling of constraint and inhibition, while a low tone produced the opposite effect. Meyer (**16**) found that an increased effort of attention produced a decrease in the size of writing and concluded that this was a special instance of a general increase in muscular tension which accompanies an effort of attention. Downey (**6**) reports an increase in pressure accompanying an increase in the difficulty of the attendant mental operation. The changes in writing with practice and facility consist, on the other hand, according to Diehl, in an increase in speed and a decrease in pressure and size. The author does not attempt to interpret these changes.

A similar investigation was carried on by Mayer (**14**). Mayer confirmed in general the results of the above-mentioned investigators and in particular the conclusions in regard to the relationship between speed, pressure and size. He attempts to explain the effect of practice upon these elements. He concludes that in the case of practice there is present a high dgree of effort and that this effort is directed toward a production of relatively high speed. In the case of unskilled writers this effort does not produce a decrease in size because they are unable to make the required forms in a small compass. With increasing skill this decrease is brought about. This explanation, however, does not take account of the fact, which is well established, that an increase in effort produces an increase in pressure. A more probable explanation would seem to be that with an increase in practice the acquirement of speed is attained with less and less effort. With increasing skill this is accomplished by economizing on size. In other words, a decrease in size may be due to other factors than increased effort. The decrease in pressure is explained by the greater ease consequent on practice.

The rhythm of writing was studied by Margaret K. Smith (**18**) and Awramoff (**2**). Miss Smith used no special apparatus, but Awramoff used one which recorded the pressure by a combination of the registering devices of Goldscheider and Kraepelin, and the speed by a system of contact lines. The pencil in passing over these contact lines successively made and broke an electric circuit. Speed determination by such means is not accurate however, since the number of contacts per unit of length varies greatly with the angle at which the stroke crosses the contact lines.

Both authors found writing to be characterized by decided rhythm. This rhythm, however, is the result of development and is characteristic to a very limited extent of the writing of children. Awramoff describes the difference by saying that adults write a letter or series of strokes with a total unitary innervation (*Gesammtinnervation*), while children write with a separate impulse for each stroke. In terms of pressure the writing of children showed an individual pressure curve for each stroke, while in the case of adults the pressure throughout a letter or word had a single point of culmination to which it rose and from which it fell.

The effect of an imposed rhythm upon writing was found to be an increase in the quantity, usually accompanied by a decrease in the quality, of the writing. That is, each writer has his natural speed at which he can write best. He may learn, however, to modify the natural rhythm to a certain degree. The effect of a well chosen imposed rhythm upon children's writing was to organize it and render it more like the writing of adults.

III. APPARATUS AND METHOD

The apparatus and method utilized in this experiment were the same as those used by the author in a preliminary experiment on writing reactions, already reported in these studies (**7**). The apparatus was designed to measure the speed and the pressure of writing or of a graphic movement. The speed was measured by a method based on the kymograph principle. That is, under the sheet of paper on which the reactor wrote there traveled a strip of paper and the record of the writing was taken on this strip through a carbon paper or, in the present experiment, a typewriter ribbon. In order to determine the speed of the strip, an electric marker was made to write tenths of a second on it. By this means the speed of the whole or any part of a letter might be accurately measured, and the duration of pauses in the movement determined.

In order to determine the details of the speed variations of writing, each letter written by the subject was divided into parts 1 mm. in length. This was done by means of a pair of sharply pointed dividers. The method is not strictly accurate but if applied with care will give results sufficiently exact for the present purpose, and the probable error of measurement was not great enough to render uncertain the conclusions which were drawn. This division of a letter gave also a basis for the measurement of size. This element of writing was not, however, treated independently, but only in its relation to the other elements of speed and pressure.

The pressure was recorded by a combination of the apparatus of Goldscheider and Kraepelin. That is, the writing was done over a small table which rested on the short arm of a lever. The movement of the longer arm was transmitted by means of a tambour and was recorded on a long strip of smoked paper which ran with approximately the same speed as that of the speed strip. Upon this pressure strip there was also a time marker indicating the same intervals as that on the speed strip. The advantage of this arrangement in comparison with those of previous investigators is that the pressure record may be accurately correlated with the speed record and thereby with the letter itself. Without this correlation it is a matter of guess work to determine what part of a letter corresponds to any particular part of a pressure curve.

An unimportant change in the author's previous method of driving the apparatus was introduced. This consisted in connecting the driving shaft with the shaft of the machine shop below the experimenting room. This had the advantage of giving a more uniform motion than that which was obtained by the electric motor used in the previous experiment. This regularity of motion made it unnecessary to be particular that the writing movement was directly under the time marker.

The details of the method of measuring the speed records is as follows. The primary record, that is, the paper on which the reactor wrote, is placed over the strip on which the speed record was taken in the same relation in which they were when the writing was done. The records then appear as shown in Figure 1.

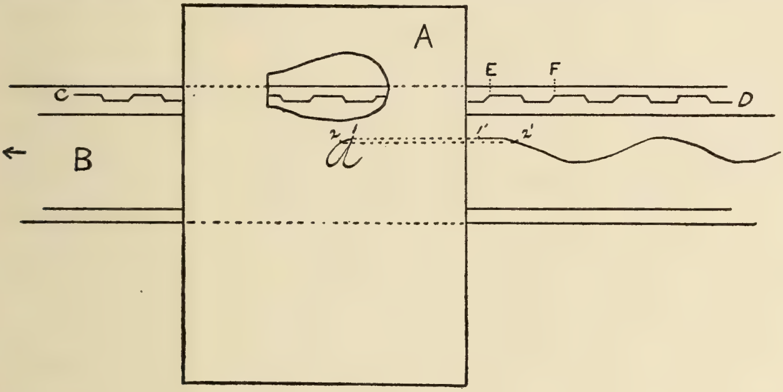

Figure 1.

The primary sheet showing the letter as written is indicated by the letter A, the speed strip by the letter B, the time line by CD, one unit of which is represented by the distance EF. The first millimeter length of the letter to be measured is represented by the distance between the points 1 and 2. The points on the time line which correspond to these two points are found by drawing horizontal lines from 1 and 2 respectively until they cross the speed curve. The line from point 1 should, of course, meet the beginning of the speed curve. If we now measure the horizontal distance between the points 1′ and 2′ we have a distance through which the strip travels during the writing of the first millimeter section of the letter. This, however, is subject to a correction due to the fact that the pencil altered its horizontal position during the writing of the section in question. Since the direction of movement of the pencil was the same as that of the strip, the amount of horizontal pencil movement must be added to the horizontal distance on the strip in order to obtain the actual distance through which the strip traveled. We now must divide this distance by the distance which represents 1/10 of a second in order to turn it into time units.

Since these operations require great accuracy of measurement, a piece of special apparatus was constructed for the purpose. This apparatus is shown in Figure 2. It consists essentially of a vertical and horizontal scale which can be applied to the records. The vertical scale consists of a sliding glass plate moved by a micrometer screw by means of which one may read to 1/50 of a millimeter. This plate, E, is fastened to the metal piece, I, which slides in a tightly fitting groove, J, in the carriage, D. The millimeter distances may be read on the edge of the piece, I, and the fractions of a millimeter on the screw. The horizontal distances are measured by sliding the glass plate with its carriage, D, along the horizontal scale, A. The latter is divided into half millimeter lengths, and by means of a vernier scale, F, divided into twenty-fifths of a centimeter, it can be adjusted to tenths of a millimeter. In order to facilitate the finer adjustment there is a supplementary carriage, G, which may be clamped to the scale. The larger carriage, D, may then be adjusted by means of the screw, H. A magnifying glass set over the scale makes the reading easier.

The primary sheet is held in place by four flat discs which can be raised from the surface of the board, in order to insert the paper, and lowered again by turning the handles, K. These operate two rods upon which are eccentric cams. The cams raise the discs which are drawn down again by springs. The glass plate slides easily over these discs. The strip containing the speed records is adjusted by means of the rod, C, and a similar one at the other end of the board.

The method of operation is as follows. The glass plate is placed above the letter upon the primary sheet. The horizontal and vertical position of the first point on the letter is then determined by reading the horizontal and vertical scales respectively.

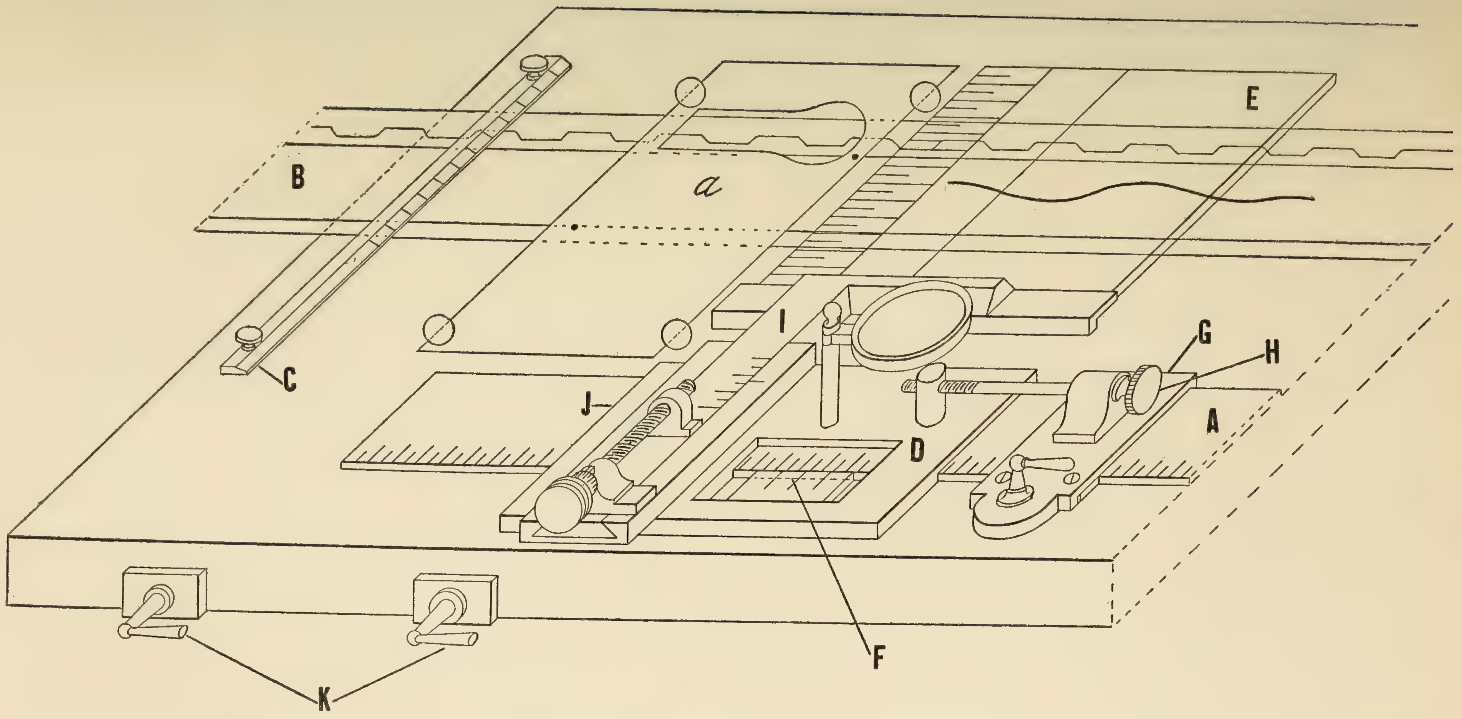

FIGURE 2—View of measuring apparatus.

The same is then done for each of the succeeding points of a letter. By means now of the vertical measurements, the points on the speed curve which correspond to the points on the letter may be found. By means of the horizontal readings the corrections for the movement of the pencil out of the vertical line may be determined. Thus, after the letter itself has been gone over, the plate is set at the first vertical reading and the point found on the speed curve which is on the same horizontal line with this. This, of course, is the beginning of the curve. The horizontal reading of this point is then taken. The plate is then set at the second vertical reading and the corresponding point found on the speed curve. The horizontal reading of this point is taken and so on for the rest of the curve. Each successive horizontal reading is subtracted from the one following it, and the differences corrected by the amount of horizontal displacement found in measuring the letter. The length of a unit of time line is then measured and the horizontal differences of the speed curve divided by this unit. The typical series of measurements is shown in Table I.

TABLE I

Illustration of the readings and operations involved in measuring the speed of the first two divisions of a letter

No. of division points	*Vertical reading on letter* *mm.*	*Horizontal reading on letter* *mm.*	*Differences in Horizontal reading* *mm.*	*Horizontal measure on strip* *mm.*	*Differences in Horizontal measure on strip* *mm.*	*Corrected differences* *mm.*
1	15.25	9.43		41.36		
2	14.45	9.38	—.05	43.42	2.06	2.11
3	14.00	9.36	—.02	44.03	.61	.63

	Dist. per 1/10 sec.	*Time per mm. in sigmas*
1		5
2	3.80	56
3	3.80	17

In order to present the results of these measurements graphically a curve is drawn, the base of which represents the successive millimeter distances of the letter and the altitude the corresponding times in thousandths of a second. Such a curve (of the letter "o") is shown in Figure 3. The interpretation of this curve indicates that the reactor took considerable time (105 σ) to get started and to traverse the first section of the letter. The pencil then moved at an average rate of 1 mm. in 17 σ until it came to the curve at the base of the letter. The section at this point took 53σ—three times as long as the average millimeter on the down stroke. The up stroke was still faster than the down stroke. The turning point at the top of the stroke was slow however, occupying 124 σ. The last outward stroke was rapid. This was the typical record of the speed changes of the movement during the writing of a letter.

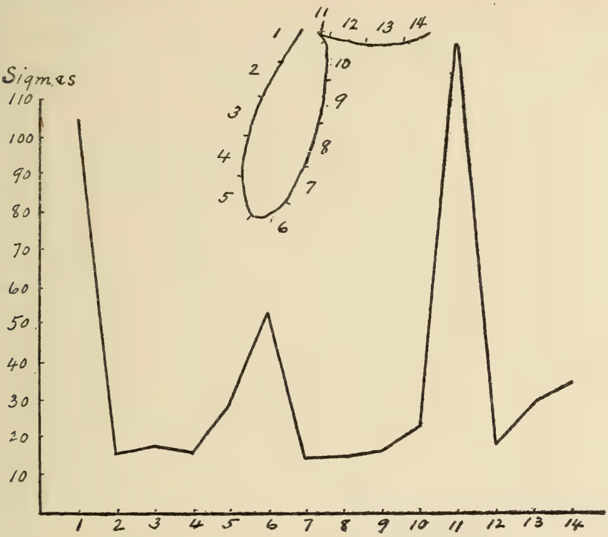

FIGURE 3—Speed curve of letter "O."

The pressure curve from the letter "o" by the same reactor is shown in Figure 8, in the appendix. The pressure curves are to be read from right to left. This curve can be correlated with the speed curve by reversing the process used in measuring a letter. In that case we found the time represented by a given distance on the strip by dividing this distance by the distance for one tenth of one second. Now in order to find the distance on the pressure record which corresponds to the time of a given section of the letter, the distance for one tenth of one second on the pressure record is multiplied by the fraction of a second which represents this time. We may thus determine the points on the pressure curve which correspond to the successive points of the written letter. In this way, in the present instance, the position on the pressure curve of three points was determined. These are the end of the first millimeter distance, the bottom turn of a letter and the top of the upstroke, corresponding to divisions 1, 6 and 11 of the letter.

From an inspection of the pressure curve it will be seen that the pressure increased during the first millimeter to almost its maximum amount. Upon the down stroke the pressure decreased. It increased again at the beginning of the upstroke but fell to the minimum about the middle of this stroke. It then steadily increased while the readjustment at the top of the stroke and the final outward stroke were being made, reaching its maximum on the latter. The drop at the end is abrupt.

The methods here used for analyzing writing movements may be briefly compared with those which have been previously used. The only other methods known to the writer by which a speed analysis of the writing movement may be made are those of the Edison pen used by Binet and Courtier (**4**), the spark method described by McAllister (**12**), and writing over contact lines described by Diehl (**5**) and Meumann (**15**). The present method possesses the advantages in comparison with that of Binet and Courtier, first, that the conditions are more normal (since the Edison pen weighs a quarter of a pound and contains machinery in rapid vibration); second, that it permits the measurement of the duration of a pause; and third, that it permits a more exact measurement of the more rapid parts of a letter. The advantages over the spark method are similar to those over the Edison pen with perhaps even greater emphasis on the first point. The defect of the system of contact lines is that the units of distance which are measured by it are not uniform. This criticism was made above.

The only method of analyzing the pressure curve with which the present method may be compared is that of Diehl and of

Meumann. The other methods of recording pressure, for example those of Goldscheider (**8**) and Kraepelin, furnish no means of determining the correspondence between the pressure and the parts of the letter. The correlation between the pressure curve and the written letter is in any case only as exact as the speed analysis of the letter. Since the latter, in the method of Diehl (**5**) and Meumann (**5** and **15**), is inaccurate the correlation of the pressure curve with the written letter is correspondingly inaccurate.

The reactors in the experiment consisted of four adults and ten children. Of the adults, three were graduate students, T. T. G., R. D. W., and F. N. F., and the fourth a college senior, D. H. Of the children, five were boys and five girls. The ages ranged from 9 to 15. C. K., 14 years, was in the second year in high school. H. H., 13 years, was in the first year in high school. M. G., 13 years, and G. B. 15 years, were in the eighth grade. O. B., 14 years, and E. C., 12 years, were in the sixth grade. C. N., 11 years, and A. S., 11 years, were in the low sixth. E. H., 10 years, was in the fourth, and M. H., 9 years, in the third grade. C. K., H. H., O. D., C. N., and A. S. were boys and M. G., G. B., E. C., E. H., and M. H. were girls. For the opportunity of working with the children the writer is indebted to Mr. Harry Houston, Supervisor of Penmanship in the New Haven Schools, who arranged to have the children come to the laboratory and was present himself at several of the experimental periods.

There were first measured two and in some cases three specimens of each letter of the alphabet of the writing of a single reactor, F. N. F. This was done in order to determine the differences and relationships between the different letters of the alphabet and thus to avoid generalizations which might rest on a study of part of the letters. Means was thus furnished also of choosing letters for use with the other subjects which might represent different typical facts in regard to their writing. From each of the remaining subjects then, records of the writing of three or four letters were obtained. In the case of adults there was some variation in the particular letters used, but with the children the same were used for all, namely, the letters "a," "r," "f," and "h." The word "American" was chosen to test the letters a and r written in combination and each reactor also wrote his own name. Mr. T. T. Giffin made a short investigation of the letters "a," "b," and "c" in his own writing in connection with the course in experimental psychology at Yale. The writer wishes to thank him for the use of the results of this study.

This may appear to be a small number of letters to measure, but the time required in the measurement of the records was so great that it was not practicable to use a larger number. It was therefore thought preferable to make an intensive and exact study of a few letters than an inexact study of a larger number. The writer plans to simplify the method of procedure without sacrificing accuracy and to extend the study over a wider range.

Besides writing in the ordinary manner it was planned that each reactor should write under varied conditions. These conditions consisted in writing much larger than usual and writing between two horizontal lines, in order to test the effect on the writing of differences in size and of the addition of certain external limiting conditions. It was also planned to study the relations between letters written separately and in combination in a word, but this purpose was only to a limited extent carried out. Reactor R. D. W. gave a complete series with the letters "r," "e," and "m" and D. H. a complete series with the letters "a," "e," and "n." Reactor T. T. G. wrote the letters "a," "b," and "c," but only in his natural writing, that is, without variations. F. N. F. gave a complete series except that he did not write between lines. The letters which were written large were "b," "r," "f," and "h," and the letters in combination (in his given name) were "r," "a," "n," and "k."

An effort was made to have all of the subjects write in as natural a manner as possible. This was probably successful in the case of adults and of some of the children. The chief tendency which had to be combated with the latter was one toward slow, painstaking writing, such as one would put in a copy book. Such writing would furnish a profitable subject of study but would be unfavorable to the type of comparison made in the present experiment. Reactors M. G., G. B., O. D., and E. C. seemed to the experimenter to write in this unnatural manner and the records of the speed of their writings was not measured in detail, only the total speed and the average speed per millimeter being taken.

IV. RESULTS AND DISCUSSION

The discussion of results will be divided into three main parts, taking up first, the average speed per millimeter of the various letters; second, the characteristic changes in speed in the different parts of the letter, which may for convenience be spoken of as the speed curve; and third, the pressure and the pressure curve. The writing of children and adults will be compared on the basis of this analysis. Under these heads also will be made a comparison of the different letters with one another, of letters written larger than usual and of those written between limiting lines.

We will first take up the consideration of the average speed of letters written under different conditions and by different adult subjects. We are dealing with the average speed per millimeter and not with the speed curve. The first result deals with the relation between the size and the average speed of different letters of the alphabet in the writing of F. N. F. The size is measured by taking the length of the line which the pencil traced in making the letter. A comparison was made by putting the letters in two lists, the first being arranged in the order of size and the second in the order of speed. These lists are given in Table II.

It will be seen that, in general, the order of the letters in the two columns is the same. That is, the larger letters are also the more rapid. The degree of correspondence may be worked out by the "r" method of correlation, according to which the degree of correlation is expressed by differences in the gradation from zero to one. The correlation in the present instance is found to be .83, which indicates a close relationship between speed and size of the letters. Some deviation from exact correspondence exists, it is true, but this may be at least partly explained by the differences in the complication of the letter and the

TABLE II

A comparison of the order of the letters on the basis of size and of speed

	Letters in the order of their average speed Reactor F. N. F.	Time in sigmas per mm.	Letters in the order of their size Reactor F. N. F.	mm.
1	j	15	k	36
2	g	17	g	36
3	f	18	f	33
4	l	20	p	33
5	k	21	h	31
6	p	21	j	30
7	d	22	t	29
8	h	22	m	27
9	z	22	q	27
10	q	23	d	26
11	t	23	y	26
12	i	24	z	26
13	y	25	b	24
14	e	27	w	21
15	m	28	l	20
16	s	30	s	18
17	w	30	u	17
18	b	31	n	17
19	v	32	v	16
20	u	33	e	16
21	x	33	a	15
22	n	35	i	14
23	o	37	x	14
24	a	38	o	12
25	c	42	r	12
26	r	42	c	11

consequent difficulty in making it. For example, the “k,” which is the largest letter, is especially slow at the last turn and this puts it lower in the speed column. Again, “j,” which is sixth in the order of size, is first in the order of speed because of its simplicity. The differences which occur may therefore be accounted for on the basis of other factors and the relation between speed and size comes out very prominently.

This correspondence between the speed and size of writing appears also from a comparison of the same letters written, first, in the ordinary size and, second, larger than usual. The comparison for the three adult subjects is shown in Table III. On the right appears the percentage increase in the size of the letters when they were written large and on the left the percentage decrease in the time per millimeter (or the increase in speed) of the large letters.

There is in these cases a decrease in the time per mm. in each case of an increase in size. The decrease in time ranges

TABLE III

Comparison of the time in sigmas per mm. of letters written normal size and large

Reactor Kind of letter	*Comparative times* per mm.					*Comparative size in mm.*			
Normal	31	42	18	22		24	12	33	31
Large	15	24	14	16		45	21	51	45
% Decrease	52	43	22	27	% Increase	88	75	55	45
D. H.		*a*	*e*	*n*		*a*	*e*	*n*	
F. N. F.	*b*	*r*	*f*	*h*		*b*	*r*	*f*	*h*
Normal		40	48	47		17	11	15	
Large		28	26	30		30	20	26	
% Decrease		30	46	36	% Increase	76	82	73	
R. D. W.		*r*	*e*	*n*		*r*	*e*	*m*	*a*
Normal		75	44	50		10	15	19	
Large		22	15	15		31	35	78	40
% Decrease		71	66	70	% Increase	210	133	310	

from 22 per cent to 71 per cent and is greater when the normal letter was unusually slow. The decrease in time is also greater the greater the increase in size. See for example, the great decrease in time in case of the reactor R. D. W.

This tendency to increase in speed with an increase of size is also shown by comparison of the total time of the letters of each sort. This comparison is made in Table IV, which refers to the same letters as those in Table III. There are four cases in which there is a decrease in the total time of a letter when the size increases. The increase in total time when it occurs is very small compared with the increase in size.

TABLE IV

Comparison of the total time in sigmas of letters of normal size and written large

F. N. F.	*b*	*r*	*f*	*h*	*D. H.*	*a*	*e*	*n*	*R. D. W.*	*r*	*e*	*n*
Normal	733	476	587	668		688	524	706		753	664	955
Large	642	487	693	675		844	515	775		679	549	1149
% Inc.	—12	2	18	1		23	—2	10		—10	—17	20

A comparison may now be made between the average speed of normal letters and of those written between lines. Only two adult reactors gave data for such a comparison. The results are shown in Table V. The lines between which the reactor wrote were so placed in the case of R. D. W. that he wrote much larger than usual. The intention of this arrangement was to prevent the reactor from disregarding the lines, but it also introduced another factor, that is, the increase in size. The amount of increase in size is shown on the right-hand side of the table. For R. D. W. this is great enough to necessitate a correction of the figures; that is, we must calculate the percentage change in time per mm. which would result from the increase in size of the letters (calculated on the basis of Table III) and by subtracting this from the actual percentage change in time get the increase which is due to the introduction of the lines. We have, therefore, in the table, first, the actual percentage change in time per mm., then the percentage change which would be produced by the increase in size and finally the corrected increase in time.

TABLE V

Comparison of time in sigmas per millimeter of letters, normal and between lines, showing increase in time.

	Time per mm.				*Size in mm.*		
R. D. W.	*e*	*m*	*r*		*e*	*m*	*r*
Normal	45	50	75		15	19	10
Between lines	49	37	41		22	45	25
% Change	+11	—26	—45	% Increase	47	137	150
% Change corresponding to inc. in size	—23	—31	—51				
Corrected change	+34	+5	+6				
D. H.	*e*	*n*	*p*		*e*	*n*	*p*
Normal	48	48	28		11	15	21
Between lines	74	73	44		11	14	32
% Increase	54	52	57				

The increase in time is very pronounced for D. H. and for the letter "e" of R. D. W. The lack of a pronounced increase for "m" and "r" of R. D. W. may be accounted for by the fact that the normal writing of this reactor was unusually slow as compared with the writing of the other reactors and with his own large writing. This then would tend to reduce the effect of lines in decreasing the speed of the writing.

We turn now to a consideration of the average speed of the writing of children. The data for discussion are shown in Tables VI and VII.

Table VI gives the time per millimeter of the normal letters written by the various children and the average for all of the children, as well as the percentage change in time of the letters written large and between lines. The corrected change in time for letters written between lines was calculated by the method already described for R. D. W.

Table VII gives the size of the letters written under the different conditions for the various children. The averages, and

TABLE VI

Time in sigmas per millimeter of the children's writing.

Reactor	Normal				Large				Between Lines				In Name	In word "American"
	a	*r*	*f*	*h*	*a*	*r*	*f*	*h*	*a*	*r*	*f*	*h*		
H. H.	65	70	37	41	40	54	40	35	55	72	43	47	41	43
C. K.	74	94	48	52	44	56	31	37					69	58
M. G.	117	118	92	98	84		55	63	117	125	75	78	105	105
G. B.	117	96	50	48	100	63	36	43	92	83	61	53	78	
O. D.	115	188	103		118	145	89	123	142	165	86	116	74	164
E. C.	174	187	184	156	181	164	117	106	182	257	125	116	124	170
C. N.	178	145	102	90		121	87	109	149	185		93	89	134
A. S.	141	152	116	115	110	85	52	62	106	130	60	52	84	125
E. H.	109	90	70	54		116	54		105	98	64	55	64	70
M. H.	47	64	42	45	52	54	36	45	84	103	64	72	71	57
Avg.	113.7	120.4	84.4	77.7	91.1	95.4	59.7	69.2	114.7	135.3	72.3	75.8	79.9	102.9
% Change..					20	21	29	11	+ 1	+12	—14	— 3		
% Change corresponding to inc. in size									—15	—13	—47	—16		
Corrected change in time									+16	+25	+33	+13		

TABLE VII

Size in millimeters of letters of the children's writing.

Reactor	Normal				Large				Between Lines				In Name	In word "American"
	a	*r*	*f*	*h*	*a*	*r*	*f*	*h*	*a*	*r*	*f*	*h*		
H. H.	12	8	25	25	28	16	30	38	31	20	53	48	60	51
C. K.	11	12	21	19	25	20	37	35					54	43
M. G.	16	11	26	26	24		38	37	23	16	39	43	20	48
G. B.	14	12	25	23	31	29	50	40	19	16	37	40	25	
O. D.	19	11	28		23	11	28	30	20	14	45	34	40	42
E. C.	14	12	23	27	23	25	32	42	22	19	36	37	24	34
C. N.	20	16	23	24		26	40	51	24	16	41	42	42	41
A. S.	16	11	28	21	21	17	36	36	27	17	54	60	21	16
E. H.	17	12	23	24		14	43		16	14	46	44	23	24
M. H.	15	12	26	26	27	18	32	35	21	17	47	49	22	13
Avg.	15.4	11.7	24.8	23.9	25.3	19.6	36.6	38.2	22.6	16.6	44.2	44.1		
% Increase					64	68	48	60	47	42	78	85		

the percentage increase in size of the letters written large and between lines. These figures are used as the basis for the calculation of the corrected change in time for letters written between lines as given in Table VI.

The speed of the children's writing is markedly less than that of adults. The speed of the "r" for example is 42 σ per mm. in the case of F. N. F. and 64 σ per mm. in the case of the most rapid children's writing. The slowest of the children's writing gives 188 σ for this letter. Similarly, the speed of "f" for F. N. F. is 18 σ and for the children 37 σ to 184 σ. For the "h" the corresponding times are 22 σ, and 41 σ to 156 σ respectively. The same relation may be found between the writing of the same letters by the other adults and that of the children; it is not necessary to make a detailed comparison. The only striking exception is the case of the "r" written by R. D. W. This is exceeded in speed by the writing of two of the children, but the large "r" of R. D. W. is more than twice as fast as any of the large "r's" written by children.

There is still a greater difference in speed in favor of adults when the letters are written large. That is, adults show a proportionally greater increase in speed with the increase in the size of the writing.

As a class then the writing of children is much slower than that of adults. There is not found in the present experiment, however, a uniform gradation in speed for the children of advancing age, although the ten children do fall into several fairly well defined groups. H. H., C. K., and G. B., the older children, are comparatively fast writers. M. G., O. D., E. C., C. N. and A. S. write more slowly while, finally, the two youngest, E. H. and M. H., write comparatively fast.[1] This anomaly of fast writing of the younger children is probably due partly to the fact that the children did not all apprehend the problem in exactly the same way. That is, some evidently aimed very much more at accuracy of form than others. Their writing had almost the appearance of the copy book style. The other children, on the other hand, wrote more fluently and carelessly. This may be due partly to a difference in teaching. A further possible cause of difference lies in the fact that the older children had reached a higher stage of development in which they were able to write a series of letters with a unified movement. Their chief advance then would be represented not by an improvement in the writing of isolated letters but of letters in a series. The present experiment deals almost entirely with the writing of isolated letters. This may not bring out the chief difference between the writing of the younger and the older children.

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[1] Other and more comprehensive tests of the writing of children in the elementary school of the University of Chicago and in the public schools of more than fifty large cities have revealed a more regular but not entirely uniform progression in speed. The speed is affected by the quality of the writing and probably also by the style of teaching adopted by the individual teacher.

The difference in speed of the letters of the alphabet due to their normal differences in size is also a factor in the writing of children. If we compare for example, the speed of the "a" and the "r" with that of the "f" and "h," we find that the latter are written much more rapidly. The difference is not so great, however, as in the case of adults. That is, there is not so much tendency in the writing of children to maintain a constant time for letters when the size varies.

The relative decrease in speed of the children's writing between lines is on the whole probably less than that in the writing of adults. It is greater than that of the adult reactor R. D. W., but this is probably, as indicated above, not a fair representative of the average change in the case of adults. In comparison with the other adult reactor, D. H., the writing of children between lines showed very much less decrease in speed.

The discussion thus far has dealt with the average speed of the letters. We now come to the analysis of the speed of the various parts of a letter, represented in the speed curves. The results are not susceptible of presentation in tabular form, but must be given by an analysis of typical individual speed curves. A comparative analysis may first be made of the characteristics of the different curves, and the relation of these characteristics to the forms of the various letters and to the different conditions of writing in the case of one individual. The comparison may then, secondly, be extended in the same way to the writing of different adults and of children. An attempt will be made in connection with the analysis of the results to make an interpretation of them by relating them to other facts of reaction and movement. The question may be raised, for example, as to how far the changes in speed are due to mechanical or to psychological factors. We will now proceed with the discussion of the writing of the individual subjects.

Figure 4, in the appendix, gives a typical speed curve of each letter of the alphabet as written by reactor F. N. F. An analysis of these curves shows that there is always considerable time consumed in starting the stroke and bringing it to its normal speed. There is also a retardation at the end of the letter. This slowness at the beginning, while characteristic of every letter, showed great variation in amount for the different letters. The duration ranges in the whole series from 51 σ in the case of the letter "j," to 128 σ in the case of "b." The variation in the speed at the beginning is shown even in different cases of the same letter. The slowness at the end is not a uniform characteristic of all of the letters.

No principle which may account for the initial variation is discoverable. If the letters are arranged in order according to the speed of the first part of the letter, this order has no evident correlation with that which is made on the basis of the size of the letter. We cannot say, therefore, that either the average speed or the size of the letter has a noticeable effect on the time of the first part of the stroke, for it will be remembered that there was a correlation found between average speed and size. The complexity of the letter might conceivably have an effect upon the speed of the beginning, and if we allow for this factor, there might be a relation between the initial speed and the size of the letter in combination with the complexity. That is, theoretically we might assume that the retardation at the beginning would be greater the smaller the letter and the greater the complexity. Even with this modification, however, the two series do not show any apparent correlation.

We may ask then what the explanation of the initial delay may be. There is, doubtless, a slight mechanical inertia which would cause a somewhat gradual increase in speed at the beginning of the letter. The great variation in the initial speed, however, shows that this factor, in the cases before us, accounts for only a small part of the phenomenon. Evidently there are cases in which the writer is prepared to make a stroke as soon as the pencil touches the paper, and in these cases inertia plays but a small part. If we turn to a possible psychological factor, we must exclude any which involves a reaction time. We might conclude, for example, that the initial period represented the time of reaction to the cue furnished by the contact of the pencil with the paper. The time of the first millimeter, however, is less than 100 σ in all but three of the letters and this obviously excludes such an interpretation. The assumption that originally such a reaction took place and that the adult habit is a survival of this procedure is probably also excluded by the fact that a time sufficiently long to admit of a reaction is not present in a nine-year-old child. If a reaction to contact-stimulus were the original basis then of this delay, it must have disappeared before that age. We can only say that the initiation of the stroke includes a movement of the pencil down upon the paper and then across its surface. A slight incoördination between the two parts of the act would account for the delay at the beginning of the letter. The lack of accurate adjustment would also account for the variation in the time of this initiatory stroke. Since the initial delay is too short to be interpreted as reaction time, we cannot speak of the initial pressure as an initiatory control, but only as a possible report control, that is, a sensation which reports the completion of an intended movement. This will be again referred to in a discussion of the pressure.

The retardation at the end of the letter is still more variable than the delay at the beginning. In fact, it is sometimes absent altogether. When it is present, however, it apparently indicates that stopping a movement may be as definite an act as starting one. It was found in the preliminary experiment (**7**), for example, that a reaction by stopping a movement was longer than one by starting. But in finishing the movement of writing a letter we have not the same sort of reaction as when a movement is stopped at a given signal since in writing a letter the end of the movement is anticipated during the writing. We have, however, on the objective side, as already indicated, and also on the subjective side, a definite fact representing the cessation of the movement. The movement does not merely stop but it is stopped. This is often connected with the consciousness of the "rounding up" of the letter, as though one would say, "so much is finished." This is, doubtless, much more characteristic of some writers than of others.

An examination of the speed curves (Fig. 4) shows that there are other points at which there is a definite retardation of the movement, and a comparison of these points with the letters themselves indicates that they come at places of change in direction of the stroke. We may, for convenience, call these places in the letter "turns," that is, points where the stroke turns a corner. Binet and Courtier (**4**) and Jack (**10**) mention these points of retardation and state as a general principle that the retardation is greater the sharper the turn, or that the speed is proportional to the radius of the curve. This rule would hold roughly if the delay in speed were due to mechanical factors. There is, doubtless, some approach to this general principle, but it does not apply in the wholesale fashion indicated by such a simple statement. There are other factors which influence greatly the amount of retardation. A measure of the difference which is due merely to the difference in angular change may perhaps be seen from a comparison of the "l" and the "t." We have here two turns which are radically different in form. The sharp turn at the top of the "t" is, to be sure, slower than the more rounded turn at the top of the "l," but the difference in this case is slight. The difference is much less than many differences in the case of strokes having the same sort of turn. Compare, for example, in this respect the first turns of the "a," "d," and "g," all of which are of the same form. The relative retardation of these is quite different.

The last example shows that we must look to other conditions than the immediate character of the stroke to explain the speed.

The part of the letter which follows any particular part under consideration is of importance. In the present instance, for example, the latter part of the letter "g," that is, the lower loop, doubtless has an effect upon the time of the first turn. This is but an example, of which many others will be found, of the fact that the act of writing a letter is a unitary thing, each part influencing every other part. In particular, the following parts of a letter influence those which precede. This is closely related to some of the results found in the preliminary reaction experiment (**7**). For example, the time of a reaction was very much lengthened when the stroke which constituted it was required to extend to a certain point, or to follow the course of some geometrical figure. In terms of adjustment, the motor mechanism had a different set according as the the later movement was of one sort or another. In psychological terms, the anticipation of a given type of later movement affected the previous attitude.

The same difference as that found in the last mentioned letters is also found in the comparison of the "i" and the "j." That is, with two turns exactly similar the amount of retardation is different.

If we compare the turns in the "m" and "n" with those in "u" and "w," a striking fact appears. Whereas, in the former, the sharp turns (at the bottom of the letters) are much slower than the rounded ones, in the case of the latter this difference disappears or is reversed. The sharp turns at the top are no slower than the rounded turns at the bottom.

The turns represent, therefore, something besides mere points requiring a mechanical readjustment of the stroke. They doubtless represent to a large degree the points of division of the letter from the point of view of the movement and the perception processes. That is, a letter is represented to us, not as a uniform whole, but as composed of certain subordinate units. If the division point between these units occurs at a turn, the mechanical tendency to a retardation will be reinforced. Otherwise, the movement will proceed past the turn with little more retardation than is produced by mechanical conditions. In the case of the "g," for example, it is very probable that the first enclosed loop, which it has in common with the "a," and the other loop which extends below the line, represents two such units of the letter. The development of these units of movement is characteristic of adult writing, as we shall see later.

In confirmation of the principle that certain turns show a retardation out of proportion to the change in direction of the stroke, we may point to the last turns in the letters "b," "f," "k," "o," "v," "w," and "s." From the point of view of form there is no reason for the extreme retardation of these turns. That is, we cannot assign a mechanical cause for it. They have the common characteristic, however, that they mark the completion of a certain definite part of the letter. In addition to this, most of them demand a rather delicate adjustment of the stroke so as to meet a certain point or line. These two factors together comprise the condition which causes the slowness of the movement. The first factor is the same as that which, according to our hypothesis, caused retardation at the end of the letter, and the second is related to the retardation of a reaction, already referred to, caused by the necessity of a later adjustment. Binet and Courtier (**4**) refer to this same fact when they state that the more conditions which are set for the movement, the slower it is.

The slowness at the second turn of the "z" may perhaps be ascribed partly to the fact that it completes one section of the letter as well as to the fact that it is immediately followed by another pronounced readjustment in direction. That is, the next stroke consists in a rather abrupt turn and the formation of a loop. The latter condition (a sharp turn followed closely by another) is still more strikingly exemplified in the letter "r." The first turn of this letter is no sharper than the top turns of the "i" and the "j," for example, but it is uniformly an extremely slow stroke. In analyzing the letter we see that this particular turn is immediately followed by two other readjustments in direction. In preparation for these, the movement makes an unusually long pause. Another example is shown in the comparative speed of the last upstroke of the "g" and the "q." In the latter it shows the delay which is produced by the more delicate adjustment which concludes it.

The effect of the succeeding part of a letter upon that which precedes is well brought out, too, by a comparison of the first turn in the letters "a," "d," "q," and "g." This turn is exactly the same in all these letters from the point of view of its external form. It may be readily seen, however, that it is progressively more rapid in the successive letters, in the order named. This corresponds to the fact that there is also a progressive increase in the size of the letters. Evidently then, the total size of the letter has an influence upon the speed of certain definite parts. The individual parts do not stand by themselves, but are united throughout the whole letter in the total movement which produces them, so that each part is influenced by the others.

We may now turn our attention to a comparison of the speed of the relatively straight strokes of the different letters. Binet and Courtier (**4**) stated that the longer the stroke, the faster was the movement which made it. This appears to be true in the examples which are before us. In the form of the above statement, however, it is susceptible of an explanation which our curves do not justify. That is, it is clear that from the mechanical standpoint a stroke might gather speed the longer it persisted. We would then have a shorter stroke and a longer one exactly similar so long as we compared identical distances. That is, the longer stroke would increase in speed in the same manner as the shorter one and then continue its increase merely because it lasted longer than the other. This, however, is not the case. The longer strokes reach their characteristic speed in the same space as the shorter ones, and this speed is greater in the former than in the latter. This is shown by the fact that the speed curves for the straighter strokes are practically level, and that those for the longer ones are on a lower level than the others. Compare, for example, the similar strokes of the letters "e" and "l," "i" and "j," "b" and "f," and "u" and "y."

If we compare the smaller and the larger letters with respect to the speed of the turns, we find here also the same relation as in the case of the straight strokes. That is, the longer letters are faster throughout. This enables us to analyze the difference which we found between the average speed of the large and the small letters. This might possibly have been due to the fact that the longer letters have a greater proportion of straight strokes as compared with the turns, and that they, therefore, have a greater average speed since the straight strokes are more rapid than the turns. This, it is true, must contribute to the difference in speed, but that it is not the whole or even the chief factor, is seen from the present analysis. That is, the elements in the letter which are strictly comparable are made in different time when the letter is large or small. In the large letter there is a quickening of the whole movement, an evident tendency to rhythmical writing in the sense that different strokes, though of differing length, tend to be made in the same time.

We may further compare the speed of the up and down strokes. Awramoff (**2**) states that down strokes are the faster. In our own preliminary experiment (**7**) the relation of the speed of vertical lines in the upward and downward direction was studied in the case of the four reactors. There was found to be a large variation. The reactors formed a series. One of them made the up strokes markedly faster, and another the down strokes faster. The two other reactors made the one or the other faster on the average, but the difference was much less. One of the two latter was the reactor whose writing is the subject of this analysis (F. N. F.). In the preliminary experiment he made the up strokes faster than the down strokes in eleven cases out of sixteen. In writing the letters in the present experiment there are some cases in which the same relation holds, but there seems to be no uniform rule. There are many special conditions which affect the speed of the up or down stroke, so that any tendency which may exist toward a greater speed of the one or the other is covered up. For example, the main down strokes of the "d," the "f" and the "p" are the faster, but this difference may be assigned to the fact that these strokes have fewer conditions to meet than the up strokes, or that they are of greater length. On the other hand, the up strokes of the "i," the "u" and the "z" are the faster. But in the case of the first two letters, these strokes are the longer, and in the case of the last one the up stroke has a freer course. There is therefore no clear evidence that either sort of stroke is more rapid than the other in the case of this reactor.

The relative speed of the strokes in the direction of the two diagonals, the upper left to lower right (represented in back-hand writing), and the upper right to lower left, is shown in the letter "x." In accordance with the principle laid down by Binet and Courtier (**4**), Woodworth (**21**), McAllister (**12**), and McMillan (**13**), the latter is much the faster.

The letters "x" and "t" furnish an opportunity for a comparison of the speed of the stroke which the pencil traces through the air in making two parts of the letter, with the speed with which it makes the stroke itself. The average time per millimeter from the end of one stroke to the beginning of the next in these two letters was measured, and is indicated in the line which joins the corresponding points on the speed curve. It appears that the average speed of this free stroke is the same as for the part of the stroke immediately preceding and following it. This seems on the face to be a contradiction of the statement of Binet and Courtier that connected writing is faster than disconnected. The facts, however, are too fragmentary upon which to base such a conclusion, and it will be also noted that the parts of the letter which precede and follow the free stroke, are not so rapid as the ordinary straight stroke. This is doubtless due to the fact that there is here a readjustment in direction. We cannot, therefore, in the present case, draw any far-reaching inference.

An examination of the records of the other adult reactors will indicate the variations which may be expected to occur within the adult type and will give a broader basis for comparison of the adult with the child type. A few speed curves from the three other adults are shown as samples in Figure 5 in the appendix: "a" by T. T. G. (curve 68) and D. H. (curve 78); "b" and "c" by T. T. G. (curves 66 and 72); "e" by D. H. (curve 74) and R. D. W. (curve 85); "m" by R. D. W. (curve 87), "n" and "p" by D. H. (curves 76 and 79).

In discussing the differences between the speed curves of the different persons the difference in shape of the letters must be taken into account. Compare, for example, the shape of the "c" as written by F. N. F. and by T. T. G. Aside from the extra stroke, the latter has a sharper turn at the bottom than the former. Compare also the "a" of D. H. with that of F. N. F. and of T. T. G. There are here three degrees of angularity, from the writing of T. T. G. at one extreme to that of D. H. at the other. We should then expect to find, other things being equal, a corresponding difference in the amount of retardation at the turns. If the differences do not correspond, other things, that is the characteristic speed variations of the writers in question, are not equal.

It should be said also in discussing the retardation of the turns of a letter, that we should regard the relative and not the absolute speed of these parts. As a measure of retardation then, we shall take the quotient of the time of a particular slow part of a stroke divided by the time of the adjacent rapid part. In making this calculation we will not use the extremely low points of the curves of T. T. G. since the time these represent is probably too short.

From a glance at the general characteristics of the speed curves of the various reactors, it is seen that they conform to those already pointed out in detail in the case of one writer[3]. We find the gradual increase in speed at the beginning, the retardation at each change in direction, and in some cases, notably in the writing of T. T. G., a retardation at the end.

If we examine the writing of the four reactors we shall see that that of one, T. T. G., is angular, that of two, D. H. and R. D. W. is rounded, and that of the fourth F. N. F., stands midway between the two extremes. Parallel to these differences there is a difference in the average speed, T. T. G. writing most rapidly, D. H. and R. D. W. most slowly, and F. N. F. at a medium rate. The relative speed of the straight strokes and the turns varies in the same order. There is a relatively greater retardation at the turns in the writing of T. T. G., least in that of D. H. and R. D. W., and an amount between these two extremes in the writing of F. N. F. That is, so far as we may judge from the the results of these few reactors, the relation between the speed of the straight strokes and the turns and the differences in speed for the different sharpness of the turns are the same when we compare the writing of different individuals as when we compare the writing of different letters written by the same individual. The same rule holds that the sharper the turn the greater the retardation. In regard to the relation between the differences in speed and the amount of retardation, however, the principle appears to be reversed. When we compare the writing of different individuals, the more rapid writing is characterized by greater retardation at the turns. That is, the difference in speed seems to be chiefly in writing the straighter strokes, whereas in the writing of a single individual an increase in speed in rapid writing occurs equally or nearly so in all parts of the letter. In other words, the same writer tends to preserve his characteristic writing rhythm at different speeds, while different writers who write at different speeds have also a different rhythm.

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[3] See p. 13.

It was remarked above that the writing of T. T. G. is characterized by a marked retardation at the end of the letter. It will also be remembered that this writer has greater retardation at the turns. We may then say that he has a general habit of pausing, or at least slowing down the strokes at the end of strokes and of letters. This may be called a particular type of the writing habit, and goes in this case, at least, with an angular style and with generally rapid writing.

As was found in the case of F. N. F., there is no noticeable difference in the writing of other adult reactors in the relative speed of the up and the down strokes.

In discussing the writing of F. N. F., we have already compared the speed characteristics of letters of different sizes, as for example, the "a" and the "p."[4] We may also compare the speed of certain letters written by the adult subjects at one time in the usual size and at another time written larger than usual. Two typical records of letters written larger than ordinarily are shown in Figure 6, the letters "r" written by F. N. F. (curve 47), and "n" written by D. H. (curve 82). These show the typical relation of letters written in this way to the ordinary writing. A comparison of these curves with those of the records of the same letters, written by the same subjects, in the usual size, in Figures 4 and 5 shows that the same difference obtains here as was noted between different letters of different sizes. That is, in writing the larger letter, whether we compare different letters of different sizes or the same letter written now small and now large, there is a marked increase in speed throughout the letter, both on the straight strokes and at the turns. In other words, a writer tends to maintain the same writing rhythm.

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[4] See pp. 17-18.

The records from writing between lines are so meager that general conclusions cannot be drawn from them. The results from the three cases which are at hand are suggestive, however, and are illustrated in the sample record from the letter "n" written between lines by D. H., Figure 6, curve 75. A marked retardation in the speed is here evident, and it is also evident that practically all the excess retardation comes at the turns, that is, at the places where the stroke approaches the limiting line.

An illustration of the effect on the speed of writing a connected series of letters in a word is given in Figure 7. This is the record of the letters "Frank," part of the given name of the reactor. A comparison with the separate letters "r," "a," and "n" may be made by turning to the curves in Figure 4. The striking thing about the connected letters is the great uniformity in the speed changes of the successive strokes as compared with the changes in the same letters written separately. The amount of retardation at the turns in particular, is practically the same for the various turns of the different strokes. This is a good example of a sort of unity or uniformity in connected writing which consists in a marked degree of rhythm throughout. This rhythm is partly independent of the external formal character of the strokes and partly influences the character of the external form by reducing the differences among the various letters.

The pressure changes in adult writing are illustrated by the reduced reproductions of typical pressure curves from F. N. F., D. H. and R. D. W. shown in Figure 8. These curves are to be read from right to left. The dots on the curves represent certain stages in the letter. The first dot on the right represents the end of the first millimeter of the stroke, the succeeding dots represent the various turns, so that the parts between the dots correspond to the different strokes.

A glance at the various pressure curves makes it evident that there are regularly two points of high pressure in the letter, namely, the beginning and the end. There are exceptions to this rule, as at the beginning of the letters "h" and "t" (curves c and f, Figure 8). These are, however, distinct exceptions. The particular curves shown to illustrate the writing of D. H. do not show a marked high pressure at the beginning. The writing of this reactor is not in general, however, an exception to the rule. This high pressure at the beginning and the end of the letter occurs whatever the direction of the stroke may happen to be. It may be an up stroke, a down stroke or a horizontal stroke. It is evidently due, then, to the position of the stroke in the letter. An explanation for the high pressure at the beginning has been suggested in discussing the retardation at this part of the letter. On account of the insufficient time, the retardation and high pressure cannot be regarded as constituting an initiatory cue or control, but are probably due to the fact that the beginning of the stroke constitutes a point of adjustment between the movement toward the paper and one along the surface of the paper. The stroke along the surface of the paper is not a second movement, the initiation of which follows upon the sensation of pressure caused by the contact of the pencil with the paper, but part of a single movement which may be interrupted for a longer or shorter period by the lack of perfect adjustment to the position of the paper. This adjustment may be more or less rapid and perfect. The more rapid and perfect it is, the less will be the retardation and excess of pressure at the beginning of the stroke.

The pressure at the end may be related to the fact that there is a progressive increase in pressure throughout the letter. This latter fact was noted by Gross (**9**). There is probably a general heightening of nervous excitation in the course of the writing. There is, however, a special increase in pressure just at the end. We found also that there is often a marked retardation at the end of the letter. The two facts are doubtless connected and are to be interpreted as meaning that ending a letter is a definite act psychologically and physiologically.

The above-mentioned pressure characteristics pertain to the letter as a whole and indicate its unity. There are also certain pressure changes which correspond to particular sorts of strokes. An examination of the pressure curves shows that there is, in general, a greater pressure on the down strokes than the up strokes. This fact was also pointed out by Gross. The rule, it is true, is not universal. There are variations between different letters and between different reactors, and limitations due to the position of the stroke in the letter. So, for example, the last stroke may be an up stroke and still have a high pressure. See the curve for the "h," curve c, Figure 8. Or the first stroke of a letter may be a down stroke but the initial pressure be so great that the subsequent part of the stroke decreases in pressure. Again, while a down stroke may in general be made with a high pressure, the last part of the stroke may decrease in pressure. Or the last part of an up stroke may increase in pressure in the same way. For this reason the division points on the curve do not always come at the crest or the valley. See the two "h's," curves b and c, Figure 8. Further, as was indicated in the preliminary experiment, there is often a slight rise in pressure at the beginning of a stroke, whether its direction be up or down. See, for example, the rise at the beginning of the "o," curve d, Figure 8. These variations are due to the relation of the different strokes to each other in the letter and are evidence of th oft-repeated fact that a letter is not merely a collection of strokes, but possesses a distinct unity.

There are also variations among different individuals in the relation of the pressure to the direction of the stroke. To R. D. W., for example, the rule applies but very slightly. There is somewhat greater pressure on the down stroke, but not a great deal. This brings us to the query why any such relationship exists and why it varies with different persons. The difference might formerly have been explained as a result of convention. But shading on the down stroke is not now commonly taught or practiced with definite intent in the United States. Such shading might, however, explain the phenomenon among writers in Germany. A probable explanation is to be found in the manner of holding the pen and in the character of the movement from the mechanical standpoint. If we observe the position of the pen as we hold it in the orthodox way, we will see that it makes an acute angle with a down stroke of the writing. A movement made in this position by turning the fingers about the middle joints would, if the pen point were not against the paper, trace an arc of a circle. The chord of this arc would not be parallel to the surface of the paper but would be inclined toward it as we make a down stroke. When the pen moves across the surface of the paper we compensate for this tendency by an extensor movement of the first joints of the fingers and a drawing up of the thumb. The tendency to make the movement in a simpler way, that is, by a simple arc-movement about the middle joint, survives, however, in the increased pressure of the stroke.

Reactor R. D. W. held his hand over on the right side so that the pencil did not make an acute angle with the down stroke, and accordingly we find that he exerts practically the same pressure on the down as on the up stroke. The variation in pressure is then probably the function of the mechanical conditions of writing due to the manner of holding the pen, and to the character of the movement in relation to the structure of the hand.

Pressure changes during writing which have been described in the case of one reactor are characteristic of the others with the exception just noted. There are certain variations in detail, but the general principles hold good.

A pressure curve of a letter written large is shown from the writing of R. D. W. and D. H., and of one written between lines by D. H. (Fig. 8). The effect of the modified conditions is substantially the same for all the reactors. When the letters are written large there is a greater difference between the maximum and minimum pressure of a letter than when they are written in the usual size. There is also more variation in the different parts. In the case of letters written between lines, the opposite effect may be seen. There is less difference in pressure in the different parts. In the latter case it is evident that greater care and closer attention is given to the form of the writing. In the former case, effort is put forth to write with greater speed, though the effort may be made without conscious attention. We may then assume, perhaps, that the attention is to some degree withdrawn from the form. If this inference is correct we may say that attention to form tends to a lack of differentiation in pressure and conversely that approach to automatic writing tends to differentiation in pressure.

In order to compare the writing of children with that of adults, a single letter is chosen, the letter "f." The speed record of this letter written in the usual way is taken from the writing of H. H., C. N., A. S., E. H. and M. H. (Fig. 9, curves 93, 104, 109, 121 and 115) and the record of the same letter written large and between lines is given for the last four reactors (Fig. 10, curves 105, 111, 123, 117 and 107, 113, 125 and 119 respectively). Pressure curves from the same letter for all five reactors are shown in Figure 11.

The most general facts in regard to the speed changes in adult writing appear also in the writing of children. There is a marked variation in speed in the different parts of the letter. The greatest speed is on the straight strokes and there is a more or less retardation at the turns. When we come to examine the children's writing in detail, however, marked differences appear. An examination of the curves from the children's writing reveals a great deal of irregularity, both between the writing of different children and in the writing of the same individual. Compare, for example, curves No. 104 and 109 from the writing of C. N. and A. S. (11 years of age) and curves Nos. 121 and 115 from the same letter written by E. H. and M. H. (10 and 9 years respectively). Although the latter two children are younger they have written the letters much faster than the first two. The children, in general, showed in the experiment a tendency to write in what appeared to be an unnatural and unusually slow manner. But in the case of the reactors here under consideration, the supervisor of penmanship, Mr. Houston, was present and encouraged them to write freely until he obtained what he thought was their normal writing. We may assume then that the characteristics of this writing were not the product of the conditions of this experiment. We must seek a more general explanation.

There are three possible explanations for this difference between the writing of the different children. In the first place, it may be due to individual variation. This, however, is improbable, for individual variation is ordinarily greater in the writing of adults than of children, and the variation in the present instance is greater than that which we have found in the case of adults. In the second place, the difference may be due to differences in the stage of development, the earlier stage being characterized by greater freedom and less care for form than the later one. A failure to develop in the direction of increase of speed might also, as remarked above, be due to the fact that attention was being turned to the connection of the letters with one another. The third possibility is that the writing of a child is more variable than that of an adult. The same child may write now slowly and carefully, and now rapidly and carelessly, according as the conditions vary or his attitude toward the situation is different. His habit, according to this view, is not yet settled, and since it is in the plastic state it is much more susceptible to outside influences. Adults are also subject to such influence, but not to the same extent. On the basis of our present knowledge of the child's writing habit the third explanation seems to the writer to be the most probable.

The great irregularity in the writing of the same individual may be seen by comparing (Fig. 9) the speed of the different strokes in the writing of H. H., curve 93 and of C. N., curve 121; and of the first part of the letter with the last part in the case of M. H., curve 115. These examples are illustrations of the fact referred to in the last paragraph, that the same child may have different types of writing. The extremely slow writing is evidently not due to the inability to make a single stroke more rapidly, but either to the difficulty of combining the strokes in a complex total movement, or to attention on the visual form to be made, thus introducing conditions which we have found to reduce the speed of the movement. These two causes doubtless are combined.

In spite of these variations, the children have, in common, one mark of uniform difference from adults. This difference is concerned with the relative speed of the straight strokes and turns. There is less retardation at the turns in the children's writing compared with the speed of the straight strokes. The form of the curve does not make this apparent; in order to get at the true relations one must compare the distance of the various parts from the base line. The difference between the writing of children and adults in the ratio of the speed of the straight strokes and the turns is then seen to be marked. Children have, then, a greater general uniformity of speed throughout the letter, so far as the influence of different kinds of strokes is concerned. That is, there is less difference on the average, between the speed of the straight strokes and the turns.

The curves 105, 111, 123 and 117 in Fig. 10 are records from C. N., A. S., E. H. and M. H. writing the same letter large. We have here evidence in confirmation of the position that the first two children are to be distinguished from the last two in the type of movement they employed. In writing large the first two modify the speed of their movement radically while the last two do not. The first two reactors, furthermore, modify the type of the movement so as to approach that of adults. That is, there is a greater difference between the speed of straight strokes and turns when the writing is large. The younger children had apparently written at about their maximum speed when they wrote the letter in the usual size. It may also be that the tendency to rhythm is less with them and their writing is therefore less modified by the increase in size.

There is a noticeable difference in the speed changes when the letters are written between the lines. The feature common to all the cases is the increased retardation at the turns where the letters meet the line or a decrease in speed on the part of the stroke approaching this point or both. In the case of A. S. the approach only is slow. A very noticeable case of the retardation at the turn is seen in curve 107 from C. N. A marked case of retardation on the approach is seen in curve 119, from M. H. The lines were so placed that the letter was written considerably larger than usual. This caused it to be written more rapidly by C. N. and A. S., particularly the latter, and this counteracted the retardation due to the lines.

These results indicate then that a restriction such as limiting lines has a similar effect on the writing of children as on that of adults, that is, a retardation, particularly at and just before the point of finer adjustment.

The pressure of children's writing is illustrated by curves from five reactors, Figure 11. The pressure records of E. H. were too dim to be read. The division points on the curve represent the same points on the letter as in Figure 8. Where there are only three division points the last out stroke of the "f" was too short to be indicated.

The mean pressure of the children's writing is less than that of the adults, as shown in the accompanying table:

Adults

F. N. F.	9.5	D. H.	15.0
R. D. W.	13.5		

Children

H. H.	2.5—B	E. C.	4.5—G
C. K.	8.0—B	O. D.	14.0—B
M. S.	6.5—G	A. S.	7.0—B
G. B.	7.0—G	M. H.	9.5—G

The units are arbitrary. The letters on the right indicate the sex and show that this was not a determining factor. Too much stress is not to be laid on this difference in mean pressure because the children in these schools were drilled in the avoidance of pressure. The low pressure of H. H., the son of the writing supervisor, is significant in this regard.

In certain respects the pressure changes of children's writing are similar to those of adults. There is a gradual increase in pressure toward the end of the letter. There is usually a rise in pressure on the down stroke and a fall on the up stroke. The curves from reactors A. S. and M. H. are exceptions to the rule in this respect.

The similarity of the records of some of the children to those of adults is also apparent. The pressure in the writing of H. H., C. K. and C. N., for example, increases and decreases in the same parts of the letter as in the typical adult writing. The entire release of pressure, however, is a mark of difference. In the writing of A. S. and M. H. the maintenance of the same degree of pressure for a continued period of time at the end of the letter is at variance with the adult type. In general then, the pressure of the children's writing varies from that of adult writing chiefly in that it is more irregular. This is evidence that the coördination has not yet attained stability and uniformity.

If we attempt to characterize the writing habit of children in comparison with that of adults, or in other words, if we describe the course of development of the writing habit so far as we may do so on the basis of the results of this experiment, we find in the first place that undeveloped writing is irregular. This irregularity is a matter of easy observation so far as the external form is concerned. We have here defined the irregularity in terms of the coördination. There is irregularity in the average speed and in the distribution of speed throughout the letter. For example, though the ratio between the speed of the straight strokes and the turns is less than in the writing of adults, though there is greater uniformity in this sense, this ratio is less constant than in adults' writing. There is greater change wrought in the coördination by changes in the outward conditions. The yet imperfectly formed coördination is unstable, having not yet acquired its fixed form.

Another characteristic of undeveloped writing which we have been able to define somewhat definitely, is the relative lack of rhythm. There is less tendency than in developed writing to increase the speed with an increase in size, whether of the same letter or of different ones. In writing letters larger than usual the adult increases the speed of the whole letter, while the child increases the speed of the straighter strokes.

We may venture to interpret the greater uniformity in speed in the writing of children as indicating that the writing process is a more continuous one than is the writing of adults. This continuity may be interpreted as giving evidence of more continuous and uniform discharge of nervous energy, or innervation. On the the mental side it doubtless corresponds to the more continuous and greater expenditure of effort throughout each part of the writing stroke; and to the application of a higher degree of attention to the visual aspects of the writing stroke during its production. This latter conclusion is supported by the experiments of Ahrens (**1**) and the observations of Schubert, whom he quotes, to the effect that the eye movements of children follow the course of the writing stroke more completely than do the eye movements of adults.

With the development of the coördination less attention is given to the visual aspects of the writing. The guidance or control of the movement must then be given over more largely to the kinesthetic sensations. This fact brings the points of motor readjustment in the writing into greater prominence. These are of course the points of division between the successive strokes. Furthermore, the control in developed writing is not only relatively less dependent on the visual and more on the kinesthetic sensations, but the attention is less directed toward controls of any sort. In other words the writing becomes more automatic in nature and comes more under the influence of the mechanical conditions of the movement. One of these conditions is the inertia which brings about a retardation in the speed of an object when the direction of its movement is altered. This factor again brings about a discontinuity in the movement by which the successive strokes are produced.

The results of this experiment, then, lead to the adoption of a point of view regarding the character of the nervous process by which the writing movement is produced which is at variance with the interpretation made by Smith (**18**) and Awramoff (**2**) on the basis of their experiments, and by Meumann (**15**) in his interpretation of children's writing. These authors hold that children's writing is a series of separate impulses or innervations (*Einzelinnervationen*), whereas in the writing of adults a number of successive strokes or letters is united by the fact that they are produced by a single impulse (*Gesammtinnervation*). On the contrary, the writing of children is less broken up into distinct strokes than that of adults. The fact that the pressure curves give evidence of at least as sustained effort as do the pressure curves of adult writing points to the same conclusion.

As there are characteristics of writing in successive stages of development which point to a breaking up of the continuity of the process, there are other facts which point to a progressive organization or unification. This appears in the more pronounced rhythm of developed writing and in the extent to which the speed and pressure of the individual strokes are modified by their relation to the other strokes which compose the letter or the word. This increase in the organization of the movement may be looked on as the positive result of the change in the object of attention, of which the negative result is the increased mechanization. The attention of the practiced writer is not narrowed to the portion of the stroke which is being made, but includes the word or group of letters of which the stroke which is being made is a part. That this inclusion in the attention of anticipated strokes influences the stroke which is in the course of being made accords with the fact which was discovered in the preliminary experiment (Freeman **8**), namely, that the speed of a reaction is modified by the anticipated later course of the movement.

This anticipation of letters and organization into groups accompanies the recognition of them as having meaning—that is, as being language symbols. The organization takes place primarily on the plane of the recognition of meaning, and secondarily in the motor process. This organization affects the series of innervations which compose the writing movement, not by making them into a total innervation, but by introducing modifications into the successive innervations. These modifications take the form of the development of rhythm, and of sundry retardations or accelerations of parts of strokes as the result of the anticipation of other strokes.

The statement that undeveloped writing is not organized into a complex habit so that the separate elements of movement can be relinquished to relative automatism does not mean that the child would not be able to make a single stroke by itself, not in a letter, much faster than he makes the parts of a letter. He could. But it is precisely the ability to make a stroke in a letter as though it were not restricted by its relationship to other strokes, and at the same time adjust it nicely to them, which requires the combination of automatism and control which the child cannot command. He must develop it by long practice, at first under the constant guidance of attention.

It would be out of place to discuss here any pedagogical applications of these principles, but one which follows directly from what has been said may be at least mentioned. It is unquestionably true, as Meumann (**15**) and Awramoff (**2**) hold, that since rhythm is one of the chief characteristics of the developed writing habit, to impose an artificial rhythm upon undeveloped writing will give it some of the characteristics of developed writing. It may do so at the expense of external form, however, if the precaution is not taken to suit the tempo to the ability and stage of development of the child.

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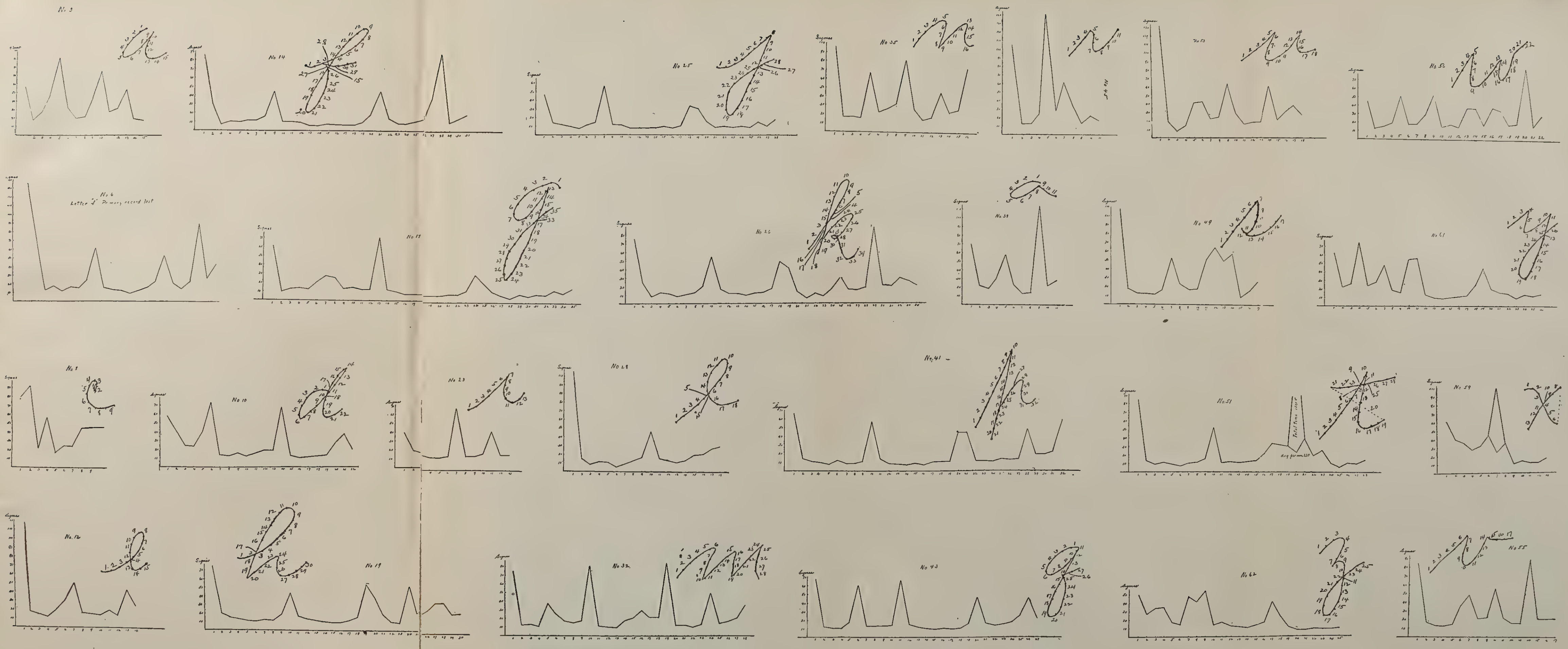


FIGURE 4—Reproducton of speed curves from all the letters of the alphabet from the writing of F. N. F.

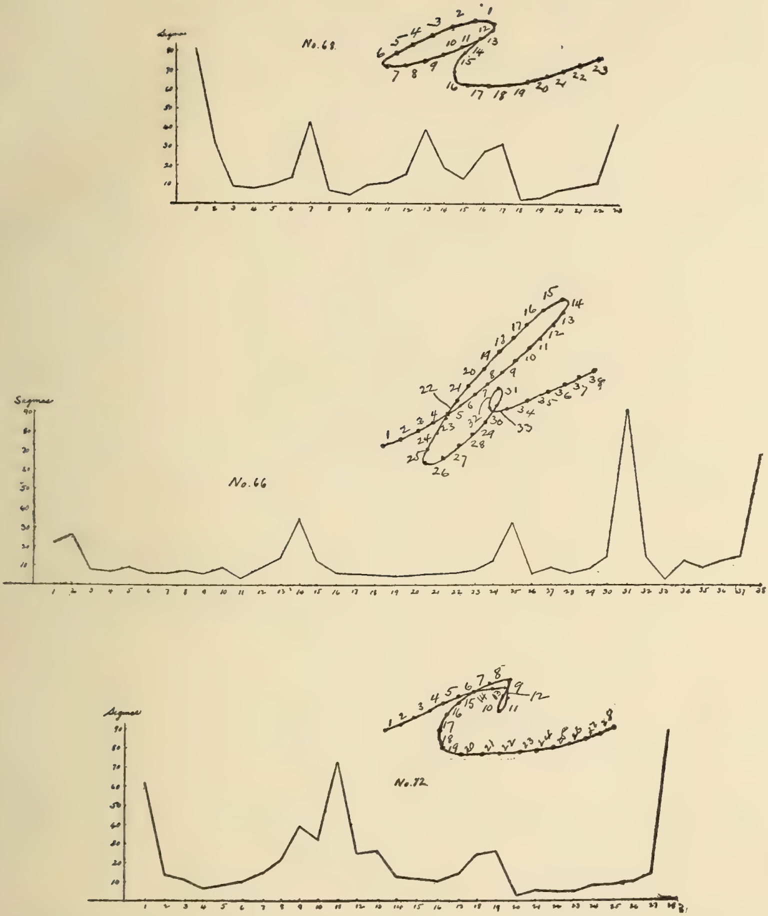


Figure 5—Reproduction of speed curves of letters written by T. T. G., D. H., and R. D. W.

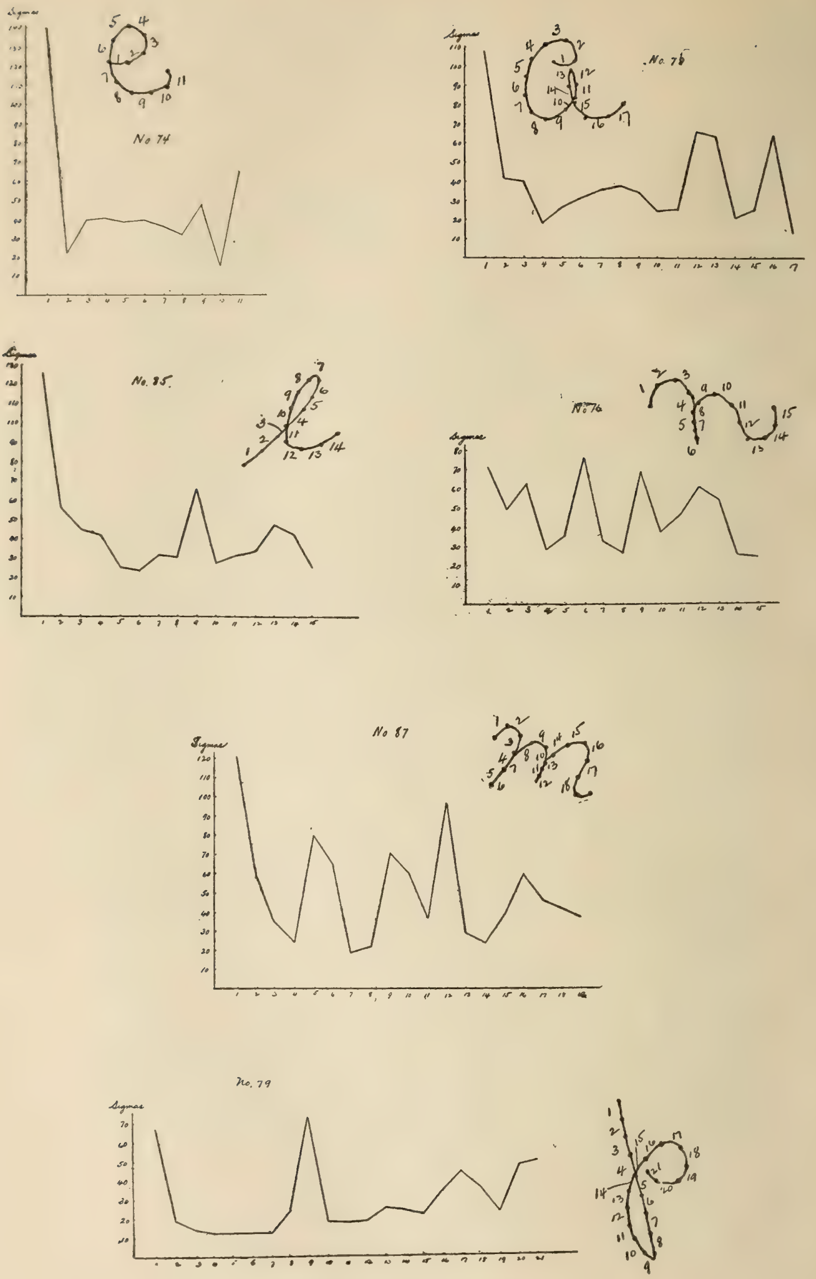


FIGURE 5 (Continued)

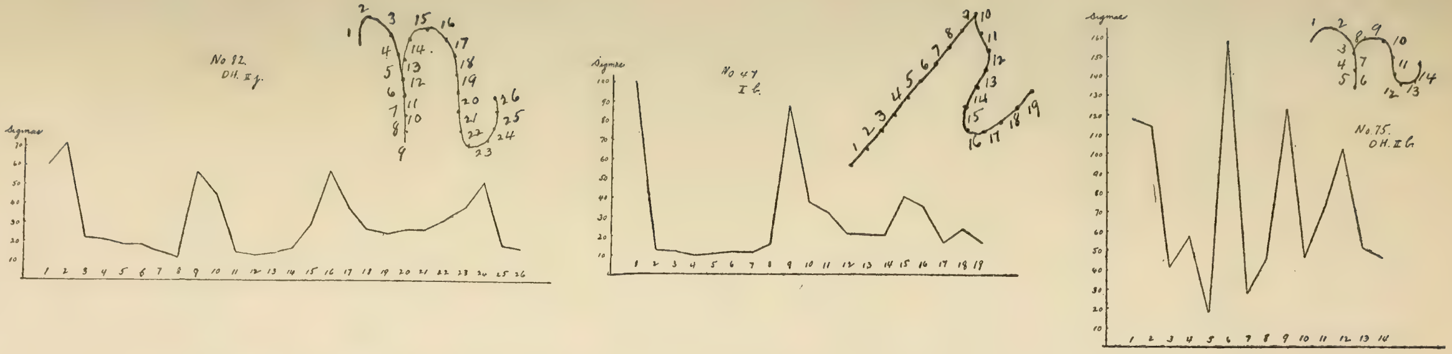


FIGURE 6—Reproduction of speed curves of letters written large by F. N. F. and D. H. (curves 47 and 82) and of a letter written between lines by D. H. (curve 75).

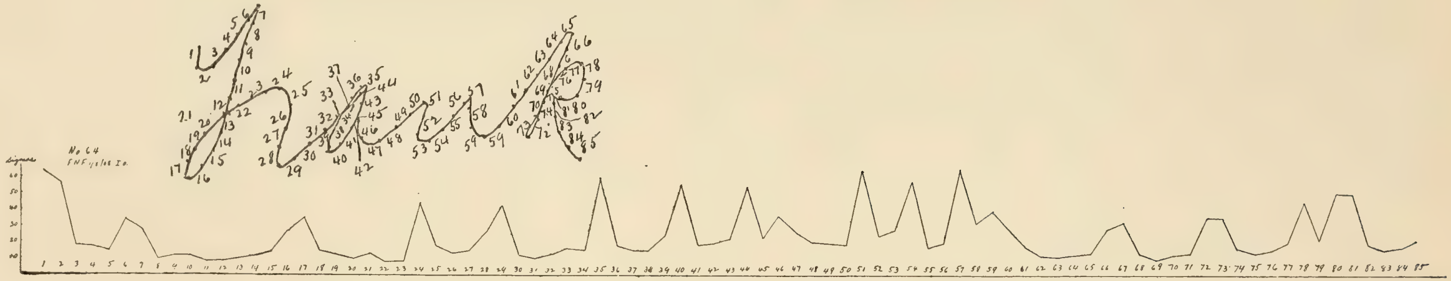


FIGURE 7—Speed curve of letters written connectedly in name by F. N. F.

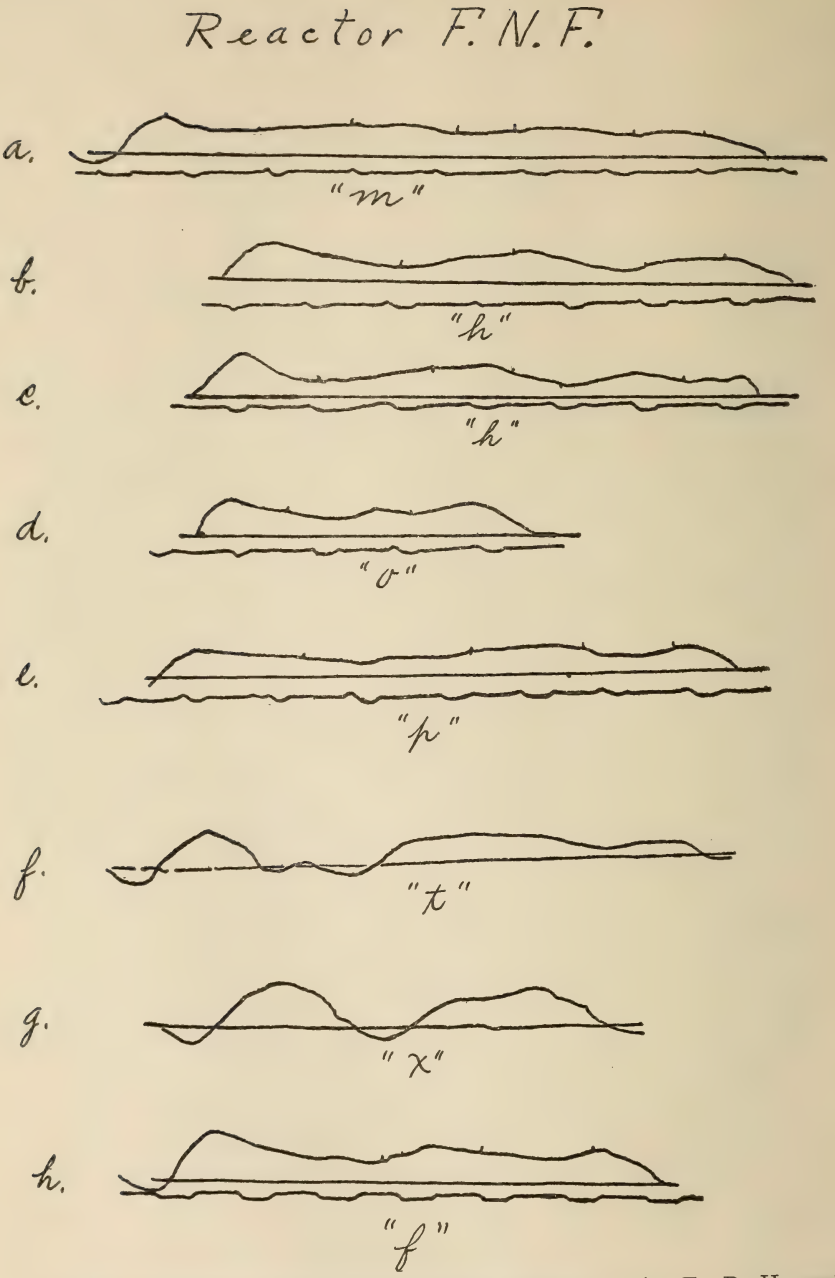
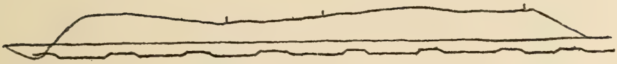
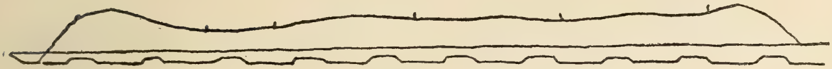


FIGURE 8—Pressure curves from the adult subjects, F. N. F., D. H., and R. D. W.

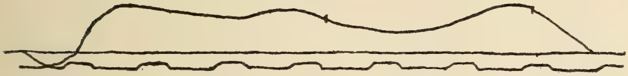
Reactor R.D.W.



"e"

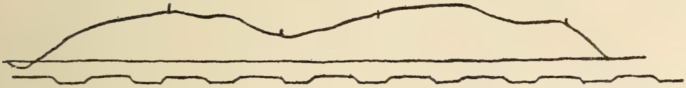


"m"

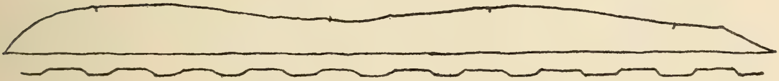


"r" (written large)

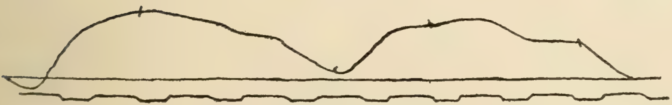
Reactor D.H.



"n"



"n" (between lines)



"n" (written large)

Figure 8 (Continued)

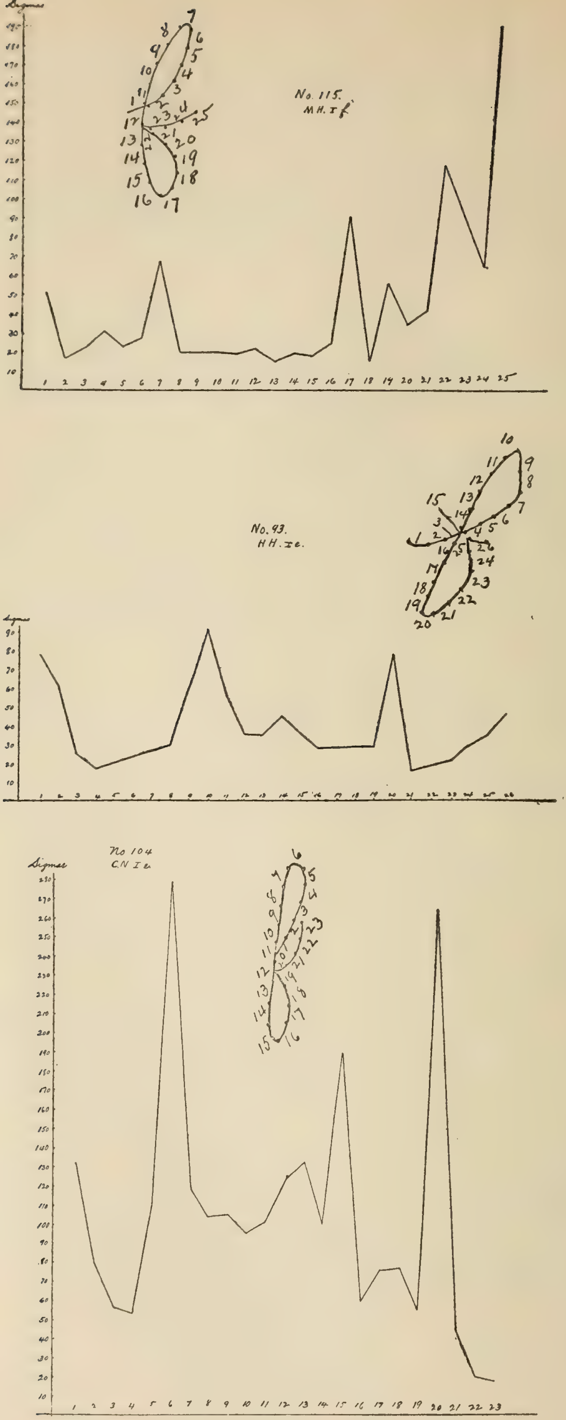
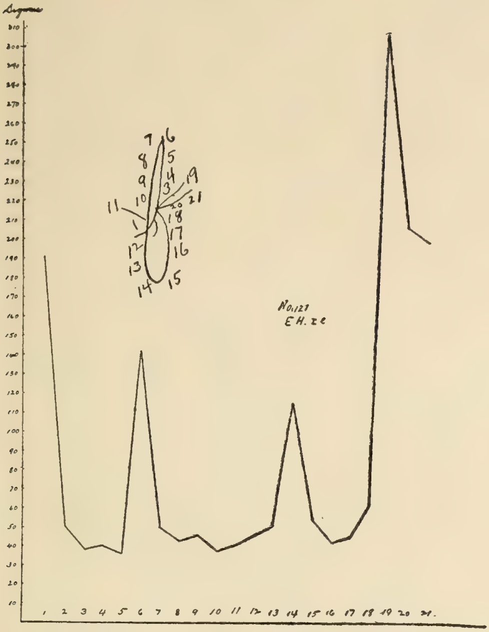


Figure 9—Speed curves of letters written by children, in usual manner



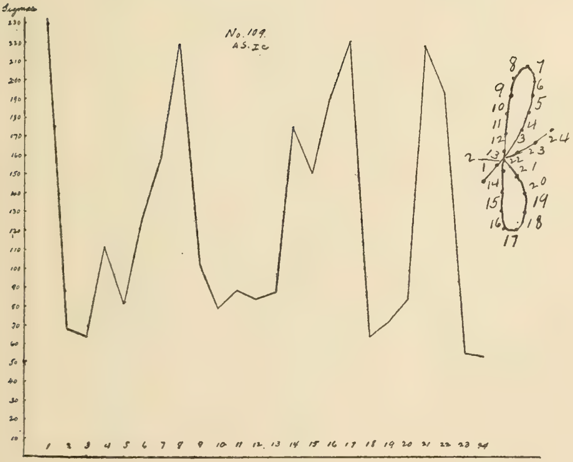


FIGURE 9 (Continued)—Speed curves of letters written by children in usual manner

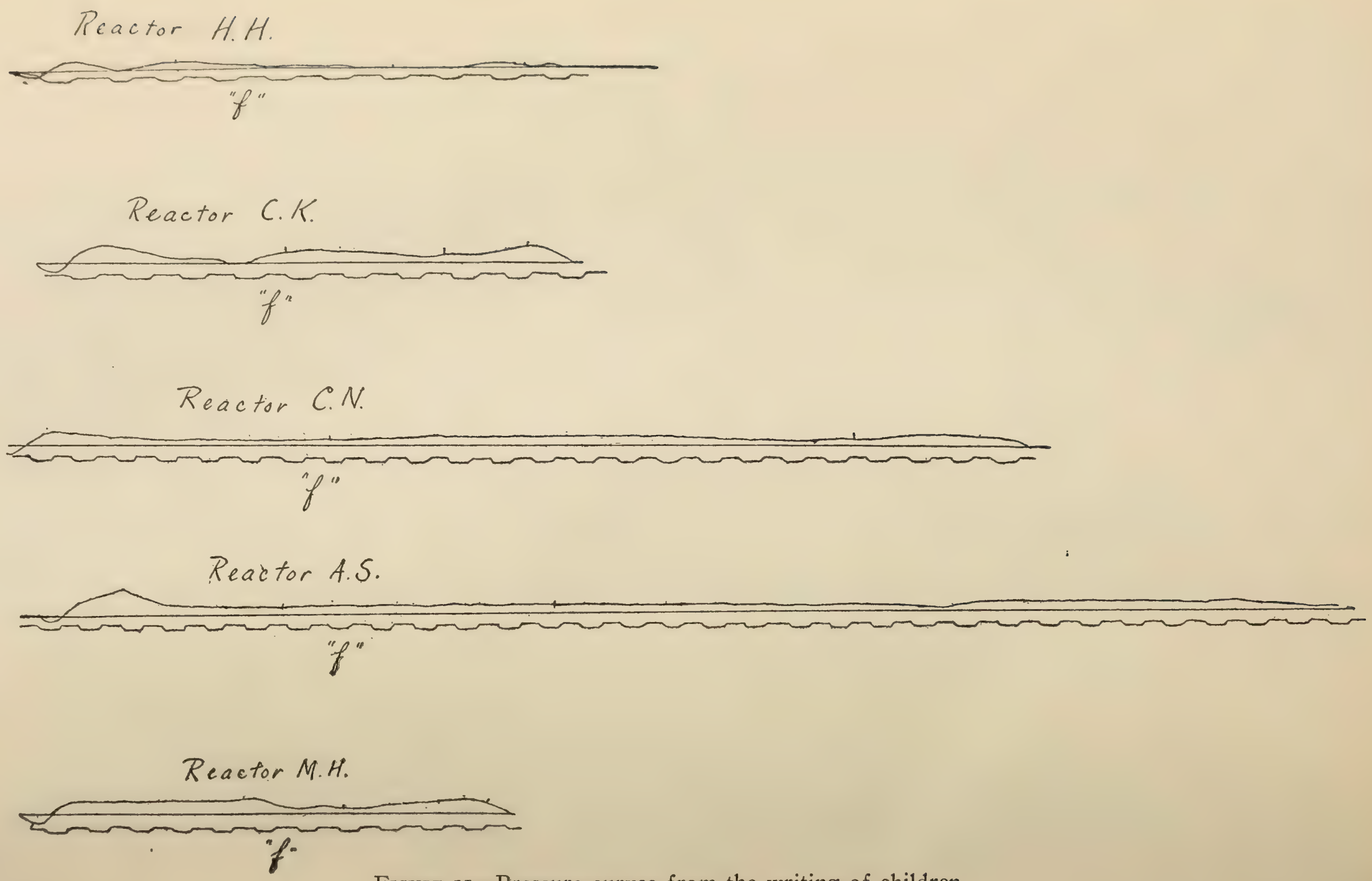


FIGURE 11—Pressure curves from the writing of children

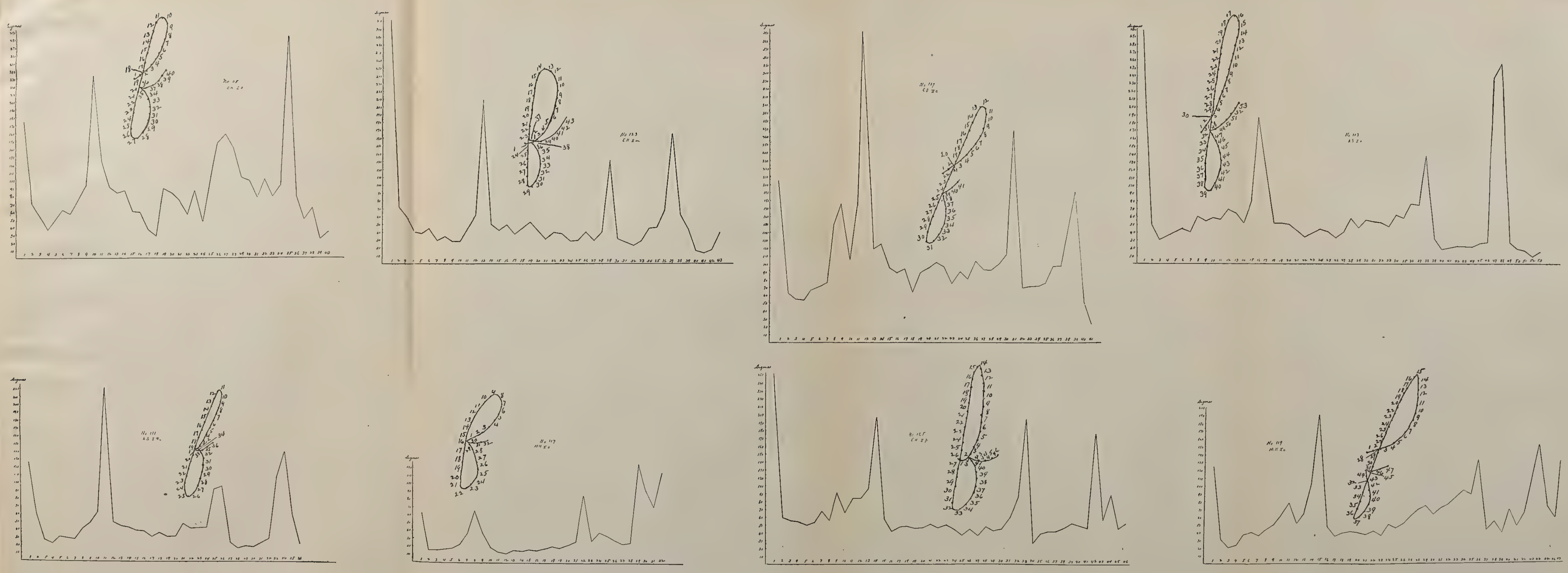


FIGURE 10—Speed curves of letters written by children, Nos. 105, 111, 123 and 117 written large, and Nos. 107, 125, 113 and 119 written between lines

EXPERIMENTAL ANALYSIS OF FORMS OF REACTION MOVEMENT[1]

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The apparatus was designed to record not only reaction times but the total form of each reaction movement as well, i.e., the speed and pressure variations from before the warning signal to the end of the reaction movement. The purpose of the whole investigation was, therefore, to study the correlations between reaction time, form of movement and introspective consciousness, particularly the direction of attention. The analysis of the conditions determining "antagonistic reactions" was specially emphasized.

The results show, in general, definite and fairly constant correlation between variations in reaction time, reaction form, and the direction of the attention. Specifically, in the typical "motor" reaction there occurred, along with shorter times and the direction of the attention to the movement to be made, a gradual decrease in the pressure of the finger on the reaction key from the warning signal to the beginning of the final reaction to the stimulus, this final movement being a simple sharp rise of the finger from the key. In the typical "sensory" reaction there occurred, along with the longer times and the direction of the attention to the stimulus, a uniform or increased pressure from the warning signal on, terminating in a final reaction movement of antagonistic form in response to the stimulus, i.e., a downward pressure of the finger before it was lifted from the key. The average correction in the reaction movement made necessary by this antagonistic phase was 30 σ.

Types of reactors also appeared, the differences in type involving the reaction time, the attention and the form of reaction movement.

I. HISTORICAL

The investigation here reported was essentially an experimental study of *forms* of reaction movements graphically recorded. In particular, the aim was to discover what correlations, if any, arise in practice series between reaction form and reaction time, or between form and the direction of attention. Since the so-called antagonistic reaction is intimately involved in these questions special study was given to it.

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[1] A condensation of a thesis submitted in partial fulfilment of the requirements of the degree of Doctor of Philosophy in Yale University, in May, 1909.

The importance of the study of forms of reaction movements as distinguished from the mere investigation of reaction times and their introspective accompaniments was first brought prominently to light by Smith (**5**) in 1903. He had noticed in earlier reaction work that a reactor, in responding to the stimulus, does not always lift his finger immediately from the key on hearing the stimulus, but often presses down before making the final upward movement. This type of reaction Smith called "antagonistic reaction movement."

In order to obtain graphic records of such a movement, Smith used a piece of stiff rubber tubing. Making one end air tight he connected the other with a recording tambour by a rubber tube. The reactor placed his finger on the stiff tubing and reacted by lifting the finger. The form of the reaction movement was thus recorded on the smoked paper of a kymograph. By means of a time line from a 100 v.d. tuning fork the time between the giving of the stimulus and the beginning of *any* movement, as well as that between the giving of the stimulus and the beginning of the *final upward* movement could be determined, the difference between them showing the time added to the reaction by the antagonistic movement.

Smith reports the results from 33 subjects, 25 men and 8 women. Of these subjects he found 5 fairly constant antagonistic reactors, while 5 were intermittent in this type of reaction. Eighteen of the subjects gave no sign of the antagonistic movement. The remaining 5 were difficult of interpretation.

The average reaction time, i.e., to the beginning of the final upward movement, for sensory reactions for 3 antagonistic subjects was 200σ, 170σ, and 180σ, respectively. Corrected to the beginning of movement, these same subjects gave 180σ, 140σ, and 140σ. The antagonistic portions of the reactions were, therefore, 20σ, 30σ, and 40σ. One result of this investigation was, certainly, to show the obvious value of the study of the form of movement in reaction experiments, and this can be done only by the graphic method of recording. The importance of this general result is emphasized by a further fact, namely, that the form of reaction may change during the course of an experiment, for Smith found that the antagonistic reaction does not always appear at the beginning of the series. It may not appear until several series have been taken.

Smith then carried on some extended experiments in order to determine the cause of the antagonistic reaction. He tried first the sensorial-muscular distinction in attention. He found that the antagonistic movement occurred in both forms of reaction, but that it was more frequent in the motor type. Experiments on the influence of distraction and fatigue on the antagonistic reaction gave no univocal results. Reactions were also taken with different sets of muscles, with different positions of the arm, and with different fingers. The antagonistic reaction movement remained under all conditions. Smith says (p. 57): "In every direction of movement, up or down, to the right or left, there were observed preliminary movements in the direction opposite to that intended by the reagent. The movements are not equal in both cases, but the curves of both persons experimented upon show the phenomenon unmistakably." It was not due, then, to any special set of muscles or to any position of those muscles.

Smith presents an explanation for the antagonistic movement both from the physiological and from the psychophysical standpoints. Physiologically, he bases his explanation on the now well known work of Sherrington, in which it is shown "that when one of the pair of antagonistic muscles is innervated, there is a simultaneous inhibition of the muscles opposed to it." With this cue, Smith reasons that the muscles already innervated would tend to persist in their activity after the stimulus and before the subsequent inhibition occurs. From the psychophysical standpoint he states that in the period preceding the occurrence of the stimulus the most prominent idea in the mind of the reactor is that of holding the finger down on the key. The stimulus comes as a kind of shock. This shock tends, he thinks, to emphasize the idea already in consciousness in the instant preceding the voluntary decision to lift the finger. He mentions certain cases of patients suffering from general paralysis in which he has noticed this preliminary tightening of the muscles before the final action.

Smith suggests a number of problems for future work, largely along the line of a more definite training of the subjects under different experimental conditions, particularly those of attention.

Other references to Smith's antagonistic reaction which I have been able to find outside the work on reaction movements done in the Yale laboratory are by Titchener (**6**, 352-355) and by Franz (**2**). Titchener, reviewing Smith's work and that of the Yale laboratory, suggests further problems which need to be worked out in connection with the antagonistic movement, e.g., the effects of practice, of definite instruction, of introspective control. He favors *a priori* the view that the antagonistic form of reaction will appear more often in the motor reactions, and states that he has tried some experiments with the press reaction, but that no antagonistic movement was found. The experiments were, however, so few in number that no general conclusions can be drawn from them. Franz found that the reaction times of a depressed or retarded patient were longer than the normal and, much to his surprise, that simple reaction times increased without any change in choice reaction times. This change was so marked that in certain series the choice reactions were shorter than the simple. Since there was no evidence that the subject had changed the character of his attention in the simple reactions, or that fatigue played any part, Franz believed that he might be dealing with the antagonistic reaction movement. Smith had already stated that he had noted preliminary contractions in partially paralytic patients. Franz made no attempt in his investigation to determine this point.

In 1905 Judd, McAllister and Steele (**3**) published an investigation on the character of reaction movements. Graphic records of both simple and discrimination reactions were taken from 51 subjects—a total of 964 records of simple reactions and 523 of discrimination reactions. A special apparatus was constructed, consisting of a reaction key attached to one end of a metal spring in such a way that when the spring was depressed the reaction circuit was made; when the spring was released the circuit was broken (pp. 143, 144). A lever attached to the spring near the key recorded every change in pressure directly on a long strip of smoked paper. Times were taken with a Hipp chronoscope.

These authors were interested not only in the nature of the reaction movement subsequent to the giving of the stimulus and terminating in a lifting of the finger, but also in the movements made in the period between the warning signal and the stimulus. They show drawings of many forms of movement, all more or less complicated, which lead to the conclusion that there is "no such thing as a simple reaction movement." All these records show, too, complex forms of adjustment in the period before the stimulus which determine the character of the final movement. For purposes of analysis the total reaction period was divided into that before the warning, that immediately following the warning, that just before the stimulus, and the final movement. An analysis of all the records yields three main types of reaction (pp. 166, 167):

1. "The balanced type," a balancing of the antagonistic muscles shown by waves in the record.

2. "The antagonistic type," in which the pressure gradually increases in the period before the stimulus or suddenly increases directly after the stimulus.

3. "The partial reaction type," in which the tendency to react overpowers the antagonistic tendency, resulting in a partial reaction before the stimulus.

The authors believe, therefore, that their results support the theory of reaction types, if by types is meant "individual tendencies even in untrained subjects to adopt a particular mode of reaction" (p. 179). The form of reaction was found to change, however, with a change in the "character of the task required" (p. 173) and it was clear that introspective evidence of the character of movement was not to be relied on (p. 180).

Concerning the relation between form of movement and reaction time (pp. 169 ff.), it was found that a gradual antagonistic pressure in the period preceding the stimulus was advantageous for quick reaction. The sudden antagonistic reactions were, however, cases of excessive effort which, properly applied, meant speed but often, improperly applied, interfered with speed. The sudden antagonistic movement did not, therefore, as regularly promote speed as did the gradual. So far as the relation between form of movement and consciousness is concerned, the gradual increase in pressure "would indicate a growing emphasis in consciousness of the necessity of holding the hand down until the stimulus arrived" (p. 181), while the sudden antagonistic movement at the end seemed to be a matter of nerve mechanism rather than the result of a conscious process (p. 182).

Finally, as problems for future work, are suggested the training of subjects in a "particular mode of reaction" and the study of the development of habits of movement (p. 177).

II. APPARATUS AND METHOD

The present investigation takes up the study of the form of reaction movements where the authors last mentioned left it and has, in general, a fourfold purpose: following their suggestions, five untrained subjects were, without special instructions, carried through a long series of reactions to see, first, if they would develop definite forms of movement; second, if during such a practice series a change in the form of movement would accompany the reduction of the reaction times and mean variations. The results of two of these reactors, reported in the original thesis, are omitted for lack of space. They agree with the results of the three reactors here reported. Five other subjects were trained to keep their attention on the stimulus or on the movement to see, third, if any differences in the form of the reaction movement accompanied the differences in the direction of the attention. The results of two of these subjects are omitted because of the small number of reactions that could be obtained from them. In their few series there appeared no differences in the form of reaction movement correlative to the differences in the direction of the attention. In all, then, the work of six subjects is reported in this article. Fourthly, special attention was paid throughout the investigation to the antagonistic form of reaction first described by Smith (**5**), in order to determine if possible the conditions that occasion it.

Graphic records were taken of all the reactions. The form of reaction apparatus was the same as that used by Judd, McAllister and Steele (**3**), a full description of which, together with a discussion of its possibilities and advantages, will be found in their article (pp. 143-145).

Some modifications in the method of recording the changes in pressure were, however, introduced. Fig. 1 shows these in side view. A thread, S, was fastened to the reaction key, M, carried up over a small pully wheel, P, and down to the end of a light bamboo rod, 12 cm. long. This rod, T′, was securely fastened to a brass rod, G, which passed through a hole in the brass block, R. On the other end of the rod, G, was attached a bamboo rod, 36 cm. long, T, which extended to the vertical recording drum and carried at its extremity a steel point which registered the changes in pressure on the smoked paper. These two rods, T and T′, thus formed an enlarging lever by means of which the changes in pressure on the key, M, and spring, I, were recorded directly, enlarged three times, on the moving paper. Because of

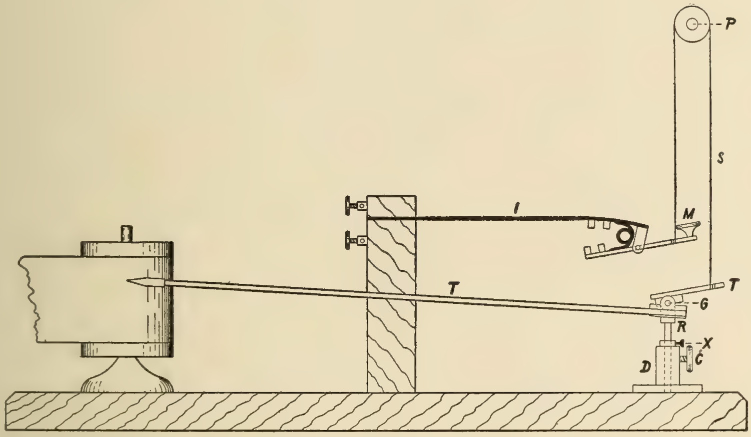

Fig. I.

the thread and pulley, there was no reversal of movement as was the case with the simple lever used in the earlier form of the apparatus. The brass block, R, was mounted on a rod which could be raised or lowered in the metal base, D, by means of the collar, X. By means of the screw, C, the amount of pressure of the pointer upon the paper could be regulated. This screw also permitted the pointer to be drawn back slightly from the paper to allow smoking and shellacking.

The records made with this form of apparatus differ materially from those taken with the earlier form, as well as from those of Smith, taken with a tambour. The advantage of the lever record over the tambour record is that with the lever the length of movement recorded corresponds directly to the amount of movement made. The advantage of this particular type of lever record over the earlier type is that, since the movement is not reversed, the form of movement can be more readily interpreted from the form of the curve.

As in the earlier work, strips of smoked paper about fifty feet long were used. The method of shellacking was also the same. It seemed best, however, to have the paper move at a uniform rate so that the changes in muscular movement occurring during the reaction could be compared by simple inspection of the curve rather than by constant reference to the time line. To attain uniform rate of movement the drums were driven by an electric motor. The motor was attached to the drum farthest from the reaction key and the proper speed was obtained by means of direct reduction through a worm gear. To do away as much as possible with the noise connected with such an arrangement, the motor was mounted on several thicknesses of rubber matting, then wrapped in many layers of thick cloth and placed in the center of a large box. This box was filled to the top with cotton waste and the whole securely covered. The residual noise of the motor, although slight, undoubtedly had, however, some distracting effect. This appeared most at the beginning of each series. Such a distraction was overcome in part by having the subject react a few times before the recorded series was begun. To any remaining distraction from the motor the subject doubtless became so well adapted that it could be considered a constant in the experiment. This was indicated by some later experiments in which the drums were turned by hand. The reactor was so much distracted by the lack of noise that it became necessary to start the motor whether it was used to turn the drums or not.

Figs. 2 and 3 show in diagrammatic form the entire apparatus and the wiring. The reaction circuit is shown in Fig. 2. The current passes from battery B1, through the 100 v.d. Koenig fork, T, to one of the break poles, 5, of the make and break key, K. In the normal position of the key the current passes across the contact, Y, to the post, 4, and so back to the battery, B1. This circuit keeps the fork in constant vibration when it is not in use in the reaction circuit. If the contact is broken at Y in the key, K, the current then passes from the battery, B1, through the fork, T, to post 5 of key K. Being unable to cross at Y, it takes the longer circuit to the electric hammer, H, through which it passes when the contact, E, is made. From the hammer the current passes through the Ewald chronoscope, C, to the reaction key, R. At R it crosses the contact, V, when

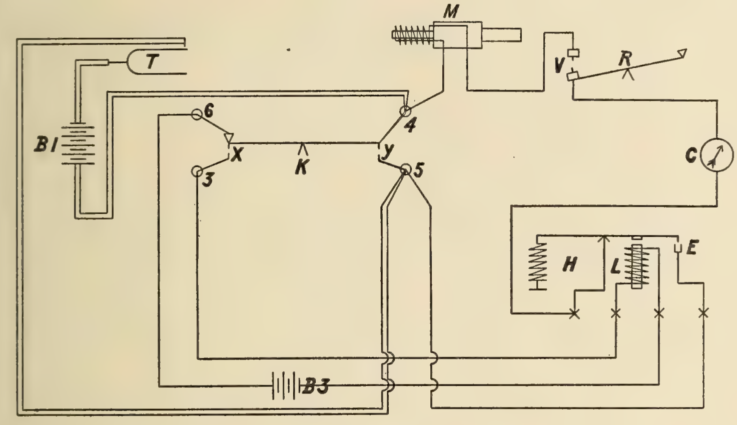

Fig. 2.

Apparatus and Connections of a Reaction Circuit.

this contact is made by the reactor, to the marker, M, and back to post 4, from which point it returns to the battery, B1.

The same movement which breaks the contact, Y, on key K, makes an instant later the contact at X. This allows the current from battery, B3, to pass from the make pole, 6, across X to the make pole, 3, through the magnets, L, of the electric hammer, H, and back to the battery, B3. When L becomes a magnet, the hammer is pulled down, giving the sound for the stimulus and completing the reaction circuit at E. The pressing of key K, then, gives the stimulus for the reaction and also completes the reaction circuit, starting the chronoscope and marker. Upon hearing the stimulus the reactor breaks the circuit at V, stopping the chronoscope and marker, after which the operator releases the key, K, allowing the fork to be run once more by means of the shorter circuit.

The tracing of a continuous time line, a procedure employed by Judd, McAllister and Steele, was not entirely satisfactory, since the marker fastened to the end of the pressure pointer made the latter heavy and unwieldy. The spark method advocated by Bliss (**1**) was also found unsatisfactory by these investigators. Both were therefore discarded in this investigation. Two considerations caused us, furthermore, to abandon the use of the Hipp chronoscope, utilized in the earlier investigation. In the first place, the object of the investigation would be fulfilled as well by a time reading which is accurate to hundredths of a second. In the second place, the use of the Hipp chronoscope made necessary the presence of a second operator. The above method kept the whole procedure under the control of a single operator.

By broken or interrupted time line, the type used in this work, is meant one in which the time is not recorded continuously, but in which the time record is started by the operator in giving the stimulus and stopped by the reactor in breaking the contact at the reaction key. The time thus recorded by the marker is the reaction time. This method does away with the necessity for one or more additional markers with the consequent error involved in dropping perpendiculars to the time line and in comparing corresponding points on the lines traced by the different markers. It also furnishes a convenient means of determining the point at which the stimulus is given and the point at which the circuit is broken by the reactor. While the marker recorded hundredths of seconds on the smoked paper, the time necessary to count these records was saved by the introduction of the Ewald chronoscope. The chronoscope and marker records were compared from time to time to make sure of their agreement.

A warning signal was used, consisting of the click of a telephone. A diagram of the warning circuit appears in Fig. 3.

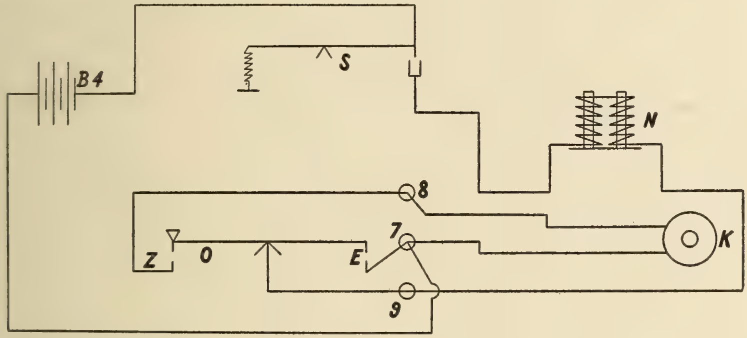

Fig. 3.

Diagram of Apparatus and Connections in the Warning Circuit.

From the battery, B4, the current passes through the outer connections of a Kronecker Interrupter, S, set by comparison with the 100 v. d. fork to vibrate at the uniform rate of ten vibrations per second. This current passes normally through the marker, N, to the post, 9, on key O, through this key to the post, 7, and so back to the battery, B4. The warning signal is given by making the connection at Z on the key, O, the same movement breaking, an instant before, the connection at E. The current then proceeds from post 9 across the connection, Z, to post 8, through the telephone, K, to post 7 and so back to the battery. When the telephone gives the warning the marker stops recording tenths of seconds. When the telephone stops, the marker starts again. In this way a record is obtained of the beginning and the end of the warning signal. It seemed best to have the length of the following interval (that between warning signal and stimulus) controlled by the operator rather than by a mechanical device giving a constant interval, since the reactor would soon become accustomed to the latter and react at the end of it without reference to the stimulus. The interval varied from one and one-half to two and one-half seconds, with an average of two and one-tenth seconds.

The subject was seated with the forefinger of the right hand on the reaction key, the hand and arm unsupported. A large curtain was hung between the apparatus and the reactor. This curtain did not interfere with the movements of the reactor, and at the same time completely concealed from him the movements of the paper and pressure pointer and the operations of the experimenter. In this way a large amount of distraction was avoided.

The procedure was as follows: The reactor pressed down the reaction key until the pointer rested at the proper place on the smoked paper. The operator then said "ready" and started the electric motor. After about one-half second the warning signal was sounded and after an interval varying from one and one-half seconds to two and one-half seconds the stimulus to react was given. As soon as the reactor had broken the circuit the motor was stopped, the chronoscope time read and recorded on the smoked paper by the operator while the reactor recorded his introspections in writing. Twenty reactions were taken at one sitting. This seldom resulted in fatiguing the reactor. On a few occasions two series of twenty reactions were taken with an interval of about fifteen minutes between.

Special care was taken with the instructions given the reactors, i.e., as to the direction of their attention and the nature of the introspections they were to record. These instructions will be considered in the discussion of the results of the individual reactors. None of the reactors except myself knew anything in regard to his reaction time or the character of his curves until the investigation was completed. In the case of my own records I knew my reaction times but did not know the character of my curves.

Typical records are given in diagrammatic form much reduced in Figs. 4 and 5. Fig. 4 shows a reaction of the gradual type, to be explained later; Fig. 5, a reaction of the antagonistic type. In both figures A represents the time line, B the pressure curve and C the warning line. The two time lines, A and C, were placed one at the top, the other at the bottom, to serve as guide lines in reading the changes in pressure. Point E on the time line, A, represents the instant at which the stimulus was given and point F, that at which the reaction occurred. The distance between these two points, E-F, is the reaction time indicated in hundredths of seconds. Point X on the warning line, C, marks the beginning of the warning signal and point Y its

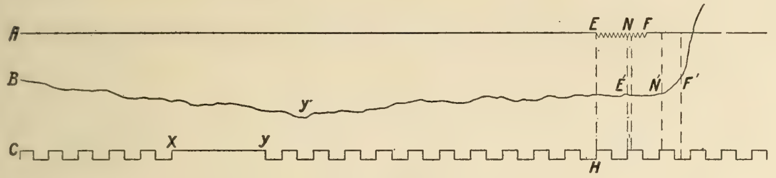

Fig. 4.

A diagram of a typical reaction of the gradual type.

A = Time line
B = Pressure curve.
C = Warning line.
X = Beginning of warning
Y = End of warning

E = Point at which the stimulus was given
F = Point at which the reaction occurs
E–F = Reaction time
Y–H = Warning interval
E' = Point on pressure curve corresponding to E.

F' = Point at which circuit was broken
N' = Point at which movement began
E–N = Corrected time.
Y' = A definite change in the direction of pressure due to the warning signal.

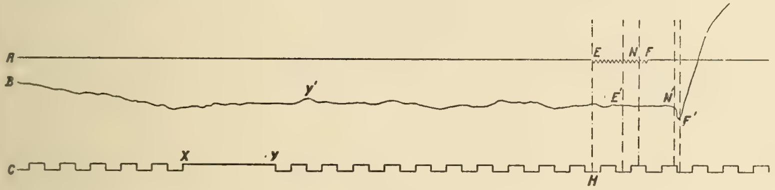

Fig. 5

A diagram of a typical reaction showing the antagonistic form

The pressure pointer is represented as 10 m.m. in advance of the other pointers.

A = Time line.
B = Pressure curve.
C = Warning line.
X = Beginning of warning.
Y = End of warning.

E = Point at which the stimulus was given.
F = Point at which the reaction occurred.
E–F = Reaction time.
Y–H = Warning interval.
Y' = A special wave due to warning.

E' = The point on the pressure curve corresponding to E.
F' = The point on the pressure curve where the circuit was broken
N' = Point at which the antagonistic reaction began.
N = Point on time line corresponding to N'.
E–N = Corrected time for the beginning of movement.

end. The two markers tracing the lines A and C were placed in line with each other. By dropping a perpendicular from the point, E, to the line, C, at H, the length of the warning interval —the time between the end of the warning signal and the stimulus—could be measured by counting the number of tenths of seconds between Y and H. In all tables the "warning time" refers to this interval. The warning signal itself lasted on an average three-tenths of a second. With the reaction key depressed, the pressure curve was traced between the two time lines. In order not to strike the time markers in its excursions, the pressure pointer was given a position not vertically in line with them, but horizontally slightly removed. In any one series of records this displacement which, of course, had to be allowed for in reading the curves, remained constant.

Since the pressure pointer did not, however, describe a line perpendicular to the two time lines, but an arc of a circle, and since the relation of this arc to the time lines was not constant throughout the investigation, it was necessary to devise an instrument for measuring at any time the horizontal displacement of the marker for any point of its arc. This is shown in Fig. 6. Two accurate steel rules, A and B, were fastened parallel to each other with an intervening space of 25 mm. by riveting them to two brass plates, P and P′. P′ is shown only by broken lines in the figure. Two other brass plates, one of which is shown at R, were grooved so that when screwed together over the rules they would slide back and forth with considerable friction. The face, OM, was made straight and perpendicular to the rules. Upon the brass plate, P′, was riveted a narrow tongue of steel, G. In the strip of brass, U, was cut a long slot, ZZ′, to fit this tongue. U could then be slid up or down in a direction perpendicular to the rules and fastened in any desired position by the screw, S. The edge, TT′, of U was straight and perpendicular to the rules, while the edge, XY, was made to correspond exactly with the arc described on the smoked paper by the pressure pointer.

At the beginning of each series of reactions a record was taken of the horizontal displacement of the arc of the pressure

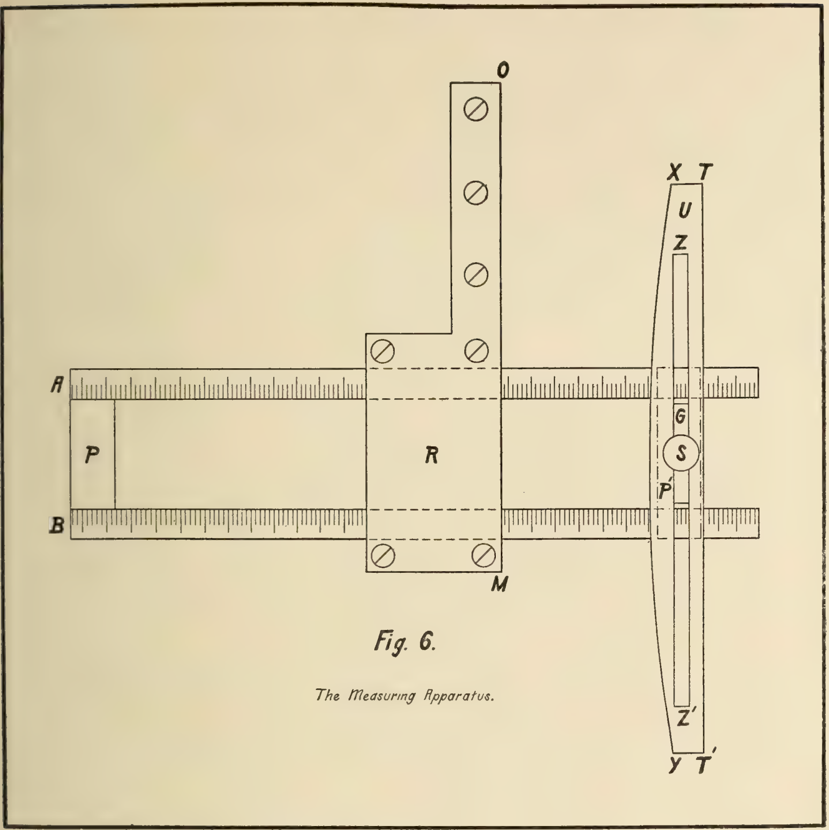

Fig. 6.

The Measuring Apparatus.

pointer from the time markers by starting the latter and moving the pointer up and down while the paper remained stationary. A record of this sort is shown in Fig. 7. To set the measuring instrument properly the upper edge of the rule, A, was squared with the line A (Fig. 7). The brass strip, U, was moved up and down until the curved face, XY, exactly coincided with the curve, XX′ (Fig. 7). The brass plate, R, was then moved towards U until the edge, OM, cut the marks, Y and Z (Fig. 7). The horizontal distance between OM and a given point on XY (Fig. 6) then represented the difference between the positions of time markers and the position of the same point on the arc of the pressure pointer. Recurring now to Fig. 4, with the instrument thus set, we may easily see its use in reading records. If the upper edge of the rule, A, is squared with the line, A (Fig. 4), in such a way that the edge, OM passes through the point, F, where the circuit was broken, the edge, XY, then represents the arc at some point of which the pressure pointer lay when the circuit was broken. This point is shown at F′. Recurring to Fig. 5, it will be seen that a similar procedure would enable one to mark on the time line the exact point, N, at which the movement began in an antagonistic reaction. Here, of course, the arc, XY, of the instrument is first brought to the point on the pressure curve, N′, where the movement began; the straight edge, OM, would then cross the time line at the synchronous point. By the same procedures points E and E′ (Figs. 4 and 5) may be determined.

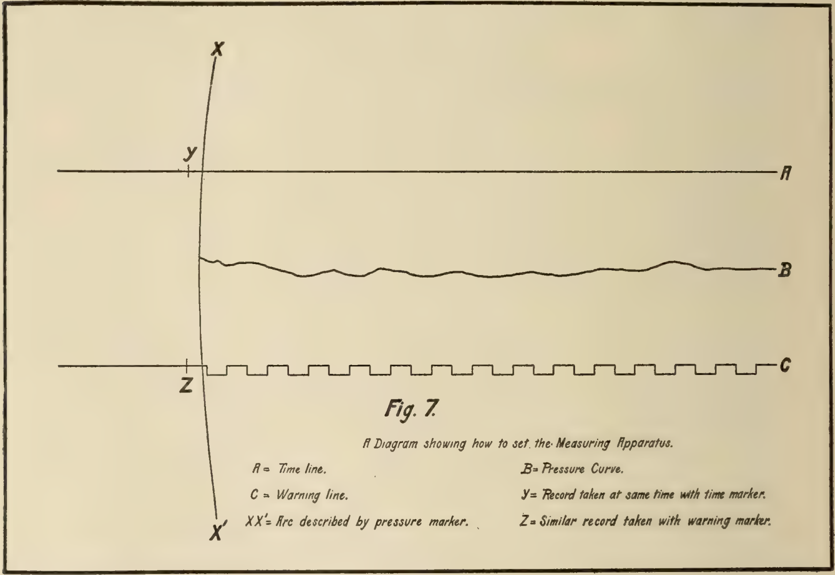

Fig. 7

A Diagram showing how to set the Measuring Apparatus.

A = Time line.
B = Pressure Curve.
C = Warning line.
Y = Record taken at same time with time marker.
XX′ = Arc described by pressure marker.
Z = Similar record taken with warning marker.

Smith (**5**) encountered difficulties just here. "In analysing and measuring the curves," he says, "it has been assumed that the apex of the antagonistic curve corresponds to the point of time at which the contact of the electric key is broken in reacting by the chronoscopic method. This assumption is perhaps not entirely correct, but if the apex be not chosen it would be difficult to find any other point on the descending curve which can be more certainly and accurately determined" (p. 51). It may be readily seen that by the use of the broken time line and some such measuring device as that described, the point on the pressure curve at which the circuit was broken can be accurately determined in any form of reaction.

Such a device does not, however, do away with the difficulty of determining the exact point at which the reaction movement itself began since, as Smith also noted, the reaction (pressure) curve often shows undulations before the real response to the stimulus begins. This difficulty will be seen more fully when we come to describe the different types of curves. It would, however, certainly be less with the spring type of reaction key than with the tambour, or rubber tube used by Smith since, in records taken with a tambour the inequalities are always somewhat softened, becoming gradual and rounded. In the spring type a sudden change in the direction or amount of pressure is indicated by a sharper change in the direction of the pressure curve.

Further trouble arose, however, here. When the measuring instrument just described was applied to the curves it was found that the point at which the circuit was broken did not correspond to the point at which the reaction movement began, even in the cases of a simple, quick, upward movement by the reactor, i.e., in reactions where there were no complicating preliminary undulations, or antagonistic movement before the upward release.

Figure 4 shows the amount of this error in a reaction of a more gradual type. The point F′ on the pressure curve represents the point at which the circuit was broken, stopping the time line. This point can be seen to be some distance up the curve. N′ represents the point where the upward reaction movement began. If we make the necessary correction backward to the time line at N, we see that in this particular reaction it would be necessary to subtract nearly one-third of the chronoscope time to find the time at which the reaction really began. This error, due to the fact that the large spring was really faster, in our experiments, than the beginning of any upward movement of any reactor, so that the spring followed the finger up for a short distance before the movement became rapid enough to break the circuit, necessitated a correction in all the reaction times. That the error was due to the large spring and not to the small spring fastened to the key itself was determined as follows: the large spring was clamped rigidly in position and a series of reactions taken with the small spring alone; the point at which the circuit was broken then corresponded exactly to the point at which the upward movement of the key began. Experiments were also tried which proved that the error was not due to the latent time of any other piece of apparatus in the reaction circuit.

This error was not a constant one, but varied directly with the rate of movement; the faster the upward movement the less the error. Since this gave a means of determining the speed of the final movement more quickly and accurately than would be possible from an inspection of the curves themselves, the error proved rather an advantage than a detriment. In the case of the more marked antagonistic reactions, the error did not appear; that is, the chronoscope time corresponded to the beginning of the upward movement of the pressure marker after the sharp downward movement. This was undoubtedly due to the sharp downward push to the spring before the finger was removed, the inertia thus given the spring allowing the finger to start upward and the small spring to break the circuit at the same instant that the large spring started upward. In a few cases of a very marked antagonistic reaction, the spring continued its downward course even after the circuit was broken.

More than twenty-seven hundred graphic records were taken in the course of the investigation. These records have been treated quantitatively and qualitatively; quantitatively as to the reaction times, qualitatively as to the character of the movements. The quantitative tables (see, as typical, Table III, p. 89) show three sets of times. The column marked "Chronoscope," includes the averages of the total reaction times (from the giving of the stimulus to the breaking of the circuit) as recorded by chronoscope and marker. The column marked "Corrected," includes the averages of the actual reaction times, or the times elapsing between the giving of the stimulus and the beginning of a sharp *upward* reaction movement. The final column of times, marked "Movement Began," records the averages of the times as corrected for the antagonistic movements. It includes also a few corrections made for gradual reaction movements and for the double type of reaction. This time, then, represents that elapsing between the giving of the stimulus and the beginning of *any* reaction movement, whether in an upward or downward direction. The tables also include the average warning time.

The quantitative results have been treated in this elaborate way in order to make possible a careful comparison between form of movement and reaction time. The custom observed by many experimenters of omitting from the average the first two reactions and any others which appeared to the reactor to be abnormal has not been followed. The character of the movement in such so-called abnormal reactions is of as much interest in an investigation of this sort as the character of the normal reactions. Only those times have been omitted from the average which were incorrect because of an error in procedure or because of extreme distraction of the reactor. Our inclusions have, of course, increased the averages and mean variations in the tables presented. The mean variations appearing in the column "Movement Began" are, furthermore, of little value for comparison with the work of other investigators since they are increased by the presence of the antagonistic form of movement. Where times and variations are referred to in the later discussion, "corrected" times are meant.

To make the reading and classification of the pressure curves possible, the record for each reaction was mounted on a separate sheet of paper. These sheets were twice the width of the records to give space at the side for notes. Mounted in this way, the records could be spread out for a quick general survey of the whole series. They could also be taken out of their order and grouped and classified according to their characteristics.

For the purpose of analyzing the form of movements, the reaction period was divided into five parts: the period before the warning signal, the period during the warning signal, the period from the warning signal to within about four-tenths of a second of the giving of the stimulus, the period from within about four-tenths of a second of the giving of the stimulus to the beginning of the reaction movement, and, lastly, the reaction movement itself. The first four parts of the reaction period were classified as to the direction and general "character" of the pressure. The final reaction movements themselves were classified according to the type of movement made. The tables summarizing the results (see, as typical, Table II, p. 77) include a key which describes the various "characters" of the movements treated. The number appearing at the top of a given vertical column of the table indicates the number of the series. A number appearing in the column itself indicates that that particular series manifested that number of times the characteristic stated in the key at the left end of the horizontal line on which the number stands. Thus, the number appearing in Table II, vertical column No. 1, opposite the rubric "no change at warning," indicates that in 5 cases in series I there was no change in the form or character of the pressure at the warning signal. Following along and horizontal line in the summary tables, from series I, one can trace the change or development, in a given pressure characteristic, through all the succeeding series. Comparing these qualitative summary tables with those showing the average reaction times for the same series, a quick judgment can be made of the relation between the form of movement and the length of the reaction time. The totals for all series are given in the column at the right side of each qualitative table. Each table also contains at the bottom a statement of the number of reactions considered in each series separately, and in all series together.

III. DETAILED DISCUSSION OF REACTORS AND REACTIONS

Reactor M.

Reactor M. was a graduate student in Yale University. He was not distracted by the apparatus even at the outset of the experiment and was able to keep his mind well upon the work in hand—facts observed not only by the operator but brought out clearly by the introspections. No instructions were given M. at the outset either as to the direction of attention or as to the rapidity of reaction. He was told to remove his finger from the key on hearing the stimulus. At the top of the first sheet used by reactor M. for introspections the operator wrote: "After each reaction introspect carefully any points which seem to you of importance in understanding the reaction." Nothing was said during M.'s series in regard to the introspections.

The arm movement made by M. in reacting during the different series was carefully watched. It was always the same—a quick upward jerk of the forearm from the elbow. In this movement neither shoulder nor wrist played any appreciable part, and hand and fingers acted practically as one.

Thirteen series, comprising 259 reactions were secured from M. without further instructions or knowledge of results. These reactions covered a period of six weeks. Eight of the thirteen series were taken in two consecutive weeks. In only one case, series II and III, was there more than one series taken in a day. In this case, series III showed such longer times and greater variations than the normal that the attempt was not repeated. Since M. (Table I) gave practically no antagonistic reactions (only 7 in all, and these of rather doubtful character), only one correction therefor was made in the time. The column marked "Corrected" corresponds, therefore, also to the time of the beginning of movement. These corrected times are incredibly fast. The reason, which will presently be discussed (see p. 83) can be understood only by reference to the character of the reaction movement.

An inspection of Table I does not show a regular decrease in the average times and mean variations due to practice, the longest times occurring to series I (92σ), III (96σ), XIII (81σ). Series III was the second series of twenty reactions taken in one afternoon. Series II, taken just before it, shows both a markedly smaller reaction time and a small mean variation. Series XIII was taken when M. was tired from examinations and lack of sleep. From series III through series IX the average time was gradually shortened from 78σ to 53σ. Series X and XI have

TABLE I.—*Summary of average times and mean variations. Reactor M.*

		Chronoscope		Corrected	
No. of Series	*Warning time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	2.3	122	13	92	13
II	2.4	100.5	10.2	67	13
III	2.0	124.4	18.1	96.5	16.8
IV	2.0	106.5	10.5	78.	13.9
V	2.3	108	17.2	72.6	22.7
VI	2.0	104.5	13.5	67.8	20.2
VII	1.9	99.1	15.3	64.4	13
VIII	2.0	97.3	13.3	67.6	13.7
IX	2.1	88.5	10.6	53.2	15.6
X	2.2	92.3	18.71	61.3	17.9
XI	2.2	99.7	16.2	67.4	25.2
XII	2.2	99.4	16.8	52.2	28.9
XIII	2.2	123	8.3	81.7	9.7
Averages	2.2	105	13.97	70.9	17.2

somewhat longer times. The time of series XII, taken when the reactor was in a hurry to catch a train, is the shortest of all the series, 52σ. M. stated at the end of this series that he thought this had not affected his times. It is apparent that the averages in these faster series are reached at the sacrifice of regularity. This is shown by a comparison of the mean variations. Series XII, with the shortest average corrected time (52σ), has the largest mean variation, 28.9. In series XIII, in which the reactor was tired and in which the average time is long (81σ) the mean variation drops as low as 9.7. Of the rest of the series, the same type of difference appears, generally, between the series giving high and those giving low reaction times.

Turning to an account of the form of the reaction movement made by M. (Table II), we find that he was a very steady reactor. The pressure curves, though showing marked changes in direction at the critical points, are very regular in appearance and quite free from minor inequalities, such as waviness. In detail, the following characteristics emerge from M.'s 259 reactions: in the period preceding the warning signal there were 150/259 cases of steady pressure (level line), 72/259 cases of increase in pressure (falling line), 31/259 cases of decrease in pressure (rising line) and practically no unsteadiness (wavy line, 6/259 cases). In the majority of cases, then, the characteristic line in this period was steadily level.

TABLE II.—*Summary of forms of movement. Reactor M.*

Key *Series No.*	*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*XI*	*XII*	*XIII*	*Total*	
Rise before warning	2	1	4	1	2	2	3	1	1	1	4	6	3	31	
Fall before warning	6	7	2	7	3	6	6	7	7	9	4	4	4	72	
Level before warning	11	10	14	12	16	11	11	12	11	10	12	8	12	150	
Wavy before warning		1				1			1			2	1	6	
Change at warning	14	13	10	15	13	16	19	14	15	17	17	16	14	192	
No change at warning	5	6	10	5	8	4	1	5	5	3	3	4	6	66	
Reacted to warning														0	
Marked rise after warning				1		1			1					3	
Medium rise after warning	2	4	1	2	2	1	2	3	3	2	1	3		26	113
Slight rise after warning	3	8	2	6	6	5	4	5	7	6	12	12	8	84	
Marked fall after warning														0	
Medium fall after warning									1	1				2	7
Slight fall after warning	1			1	1			1	1					5	
Level after warning	13	7	17	9	11	13	14	11	8	11	7	2	9	132	
Wavy after warning				1	1							3	3	8	
Marked rise towards end		4	2		1	5	1	1	2	1	1	9		27	
Medium rise towards end		4	1	5	2	7	5	2	7	7	8	5	5	58	146
Slight rise towards end	7	1	4	4	8	4	4	5	5	2	6	3	8	61	
Marked fall towards end														0	
Medium fall towards end		1					1			1				3	7
Slight fall towards end							1	2		1				4	
Level towards end	10	8	11	9	8	2	7	7	6	6	5		6	83	
Wavy towards end	2			3	2	2	1	2		3	2	9	1	27	
Reacted before stimulus			2				1	1		1		1	1	7	
Sharp reaction	19	17	20	20	19	17	18	16	18	19	18	12	20	233	
Gradual reaction		1				2	2	1			1	6		13	
Slight antagonistic reaction					1			1		1	1			4	
Marked antagonistic reaction														0	7
Gradual antagonistic reaction									2			1		3	
Double reaction		1			1	1		1				1		5	
Number of reactions in series	19	19	20	20	21	20	20	20	20	20	20	20	20	259	

The warning signal produced a change in the direction of the curve in 192/259 cases, the steady line altering into a gradual rise, which indicates a gradual decrease in pressure. From the warning signal on through the next second and a half the cases are about equally divided between an even pressure (132/259 cases) and a slight or medium decrease in pressure (113/259 cases). There were few cases either of a marked decrease in pressure during this interval (3/259 cases) or of a marked increase (7/259 cases).

In the period covered by the last one third to one half second before the final reaction movement[1] the most marked characteristic is a decrease in pressure (146/259 cases). These cases are about evenly divided between a slight rise (61/259 cases) and a medium rise (58/259 cases) towards the end,[1] except for 27/259 cases of a very marked rise. In 83/259 cases an even pressure is maintained towards the end. The proportions have changed, however, since the period before, where steady pressure predominated. There are almost no cases of increased pressure towards the end (7/259 cases), and a few of unsteadiness, (27/259 cases).

The final reaction movement itself was in almost every case (233/259) of the sharp simple type shown in Fig. 8, type A. The drawings of the curves in this and the later Figs. 11, 13 and 16 are pantograph reproductions much reduced. The two arrows to the left, on the pressure curves, represent the beginning and the end of the warning signal; the two to the right represent the points at which the stimulus was given and the circuit broken, respectively. Aside from these reactions of type A, Fig. 8, there are a few cases of the gradual type (13/259) shown in type B. These gradual reaction movements occur in the case of M. in the fast rather than in the very slow reactions, and an examination of the curve shows that the reaction was begun before the stimulus was given and then gradually completed. There are no true

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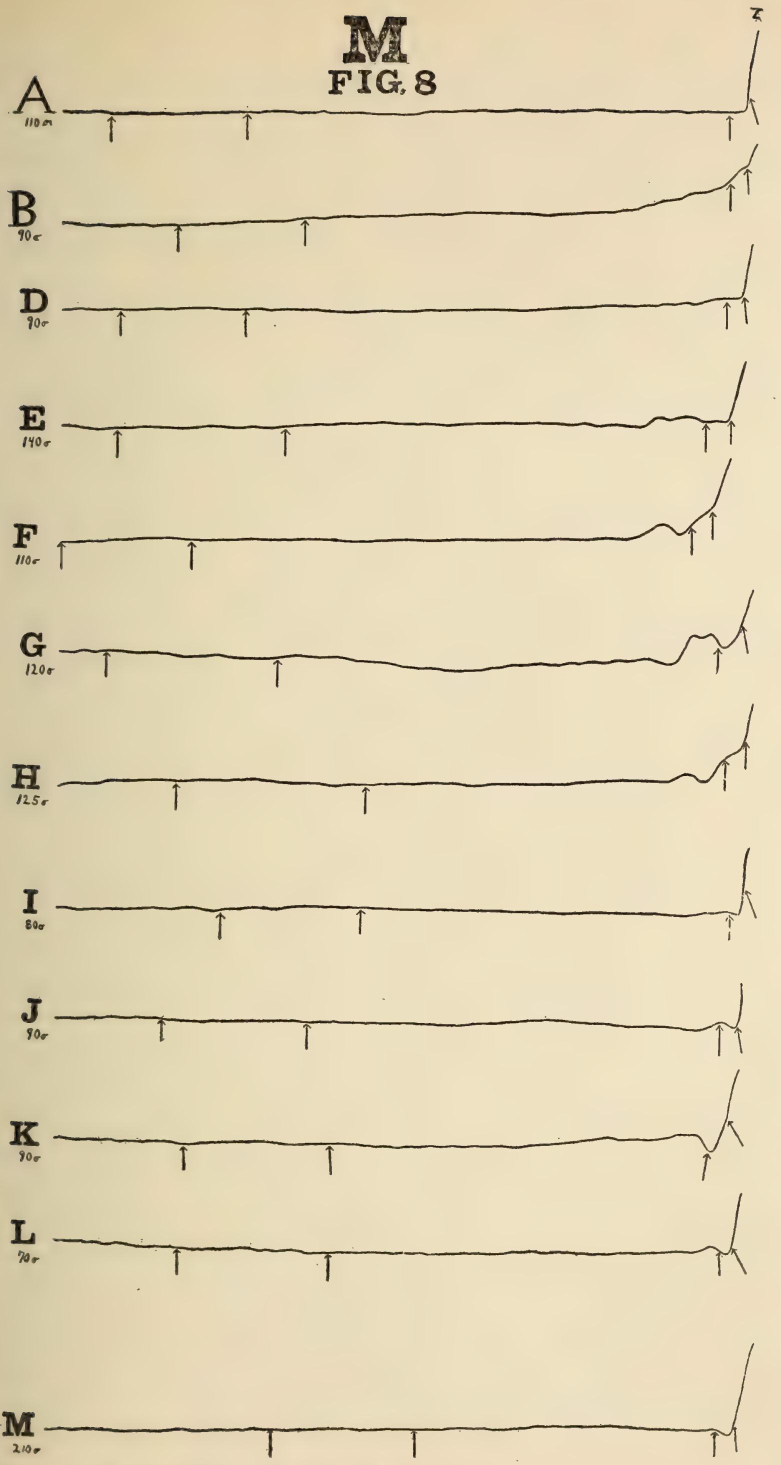
[1] By "final reaction movement" is meant the reaction to the *stimulus*. The phrase "towards the end," repeatedly occurring, refers to that part of the pressure curve just preceding the beginning of the "final reaction movement."

M

FIG. 8



Reactor M.—Pantograph reductions of pressure curves. The letters stand for the types as described in the text. The numbers under the letters represent the reaction time.

antagonistic reactions, although 7/259 cases have the antagonistic appearance. Four of these are shown in types I, J, K, and L and will be explained presently. There are five reactions of the double kind shown in types F, G, and H.

The curves give, as a whole, clear evidence that reactor M. exhibited a definite type of reaction movement in these series. Furthermore, M.'s type of movement appeared *naturally,* without any instructions and without his knowing anything about the form of movement he was making. The uniform type of the final reaction movement, which appeared in all but 26 of the 259 cases, is itself a most striking fact. Can we explain the definite characteristics of M.'s form of movement in any way, or relate them to the reaction times? Is there, further, a definite development of this form of movement during the series, that is, are the characteristics shown in the summary more marked in the later than in the earlier series?

A careful study of Table II does not show any such development through practice; there appears no steady or regular increase or decrease in the number of any of the characteristic parts of the pressure curve during the various series. A similar fact was also found for M.'s reaction times. A definite relation between form of movement and reaction time appears at the outset and is maintained quite constantly throughout all the series. Comparisons of earlier with later series will make this clear. In series II, with a short average time, there were 12 cases of a rise after the warning. In series I and III, with longer average times, only 5 and 3 such cases respectively appear. In the period just before the final reaction similar relationships occur. In the last series, XIII, which shows long times, there are no cases of a marked rise preceding the final movement. Series XII, moreover, with a much shorter average time, has 9 cases of a marked rise at this point. This relation between the time and the character of the movement towards the end of the pressure curve is shown by the curve plotted in Fig. 9. Here the upper curve, A, represents the changes in corrected time through the series plotted in sigmas, while the lower curve, B, represents the number of rises towards the end, just preceding the final

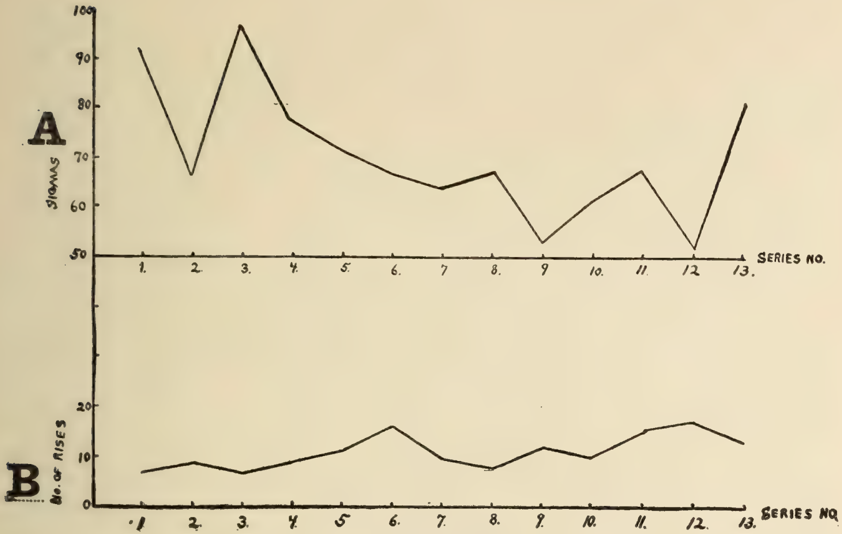


Fig. 9

Reactor M.—Showing the relation between the number of rises in pressure curves towards the end of the records and the length of the reaction time. Curve A. represents the reaction times through the series. Curve B. represents the frequency of the rises "towards the end."

reaction movement. In almost every case it can be seen that as the time curve rises, the curve showing the number of rises towards the end, drops. Where the time curve drops the curve showing the number of rises towards the end rises.

If we compare the character of the movement in each reaction with the introspections we find that the sharper the rise in the line from the warning signal on to the end, the stronger the attention. M. stated that this stronger attention was always associated with a tense muscular preparation. This increase in preparation, with the corresponding decrease in pressure, was particularly evident just after the warning signal and towards the end of the reaction period. These rises towards the end show a strained kinesthetic expectation of the stimulus. The lower curve, then, in Fig. 9, represents the degree of expectancy or of anticipation of the coming stimulus. We can better understand these characteristic features of M.'s pressure curves if we express the facts in a somewhat different way. In the period before the warning M. sat passively holding down the key with the muscles innervated only to the extent of accomplishing this purpose. The warning signal started a definite muscular preparation, shown by an increasing muscular strain not only in the muscles of the hand and arm but in those all over the body. This increasing strain was usually accompanied by a slight rise of the forearm. As the time for the expected stimulus approached, this upward movement became more marked and at last the reaction took place unless it was checked by a voluntary effort on the part of the reactor.

This muscular preparation, if partially controlled, may give rise to wavy lines as in type E, Fig. 8. It may complete itself by a medium or marked rise towards the end, as in D and B. It may go so far as partially to begin the reaction movement before the stimulus occurs, a fact then realized by the reactor and checked in time to allow for a second reaction when the stimulus comes (reactions of the "double type"), as in F, G and H. In H, it can be seen that even a second reaction had begun before the stimulus was given. If we compare these types with the introspections we find for type F: "Started before stimulus." For G: "Slow. Attempted to react too soon. Warning period seemed long." For H: "Slow. Came near reacting." These then are strong instances of checked premature reactions.

To understand the few reactions in M's series which appear to be antagonistic, types I, J, K and L, we will refer once more to the introspections. For I we find: "Tendency to react"; for J: "A forced effort to be calm"; for K: "Hard to hold down. So much energy expended in holding down, not enough left for reacting"; for L: "Nearly reacted before stimulus." These forms then are closely allied to the double type. One would suspect that, had the stimulus been delayed an instant longer in such a form as type K, it would have given rise to such forms as F. or H. These apparent antagonistic reactions occurred when the anticipatory reaction was realized and the reactor attempted to control or check it. In each of these cases the movement was not the effect of the stimulus but had begun before it, as in type K, or too soon after to be explained by it, as in types I, J, and L. Although the reactor was aware of the muscular preparation and the impulse to react, he was not aware of the upward movement of the arm in the period preceding the stimulus and was much surprised at this when shown the curves after all the series had been taken.

The reason for the very fast corrected reaction times may now be better understood (see p. 75). We found M's typical form of final reaction to be the sharp upward movement. When, however, there was a high degree of muscular preparation before the stimulus, this typical reaction resembled more the gradual type (see p. 78) because of the pronounced rise of the line preceding the final reaction movement. This is shown in the difference between types A and B, Fig. 8. In this latter type where did the reaction movement begin? Certainly before the stimulus occurred, though the gradual upward movement did not break the circuit. Such an upward movement would have completed itself with or without the stimulus, unless the whole character of the attention had been changed and directed to an effort to hold the finger down. It is evident then that the reaction time, in this gradual upward type of movement, would depend on the point of the rise at which the stimulus occurred. In some cases the stimulus came so late that the reaction was nearly completed, even the "Chronoscope" time being therefore very short, at times as low as 60σ. If in such a reaction the time is corrected to the beginning of movement it would be reduced to zero. Even in type A curves (which formed the bulk of M.'s reaction records) there is a steady rise in the curve before the stimulus, giving short reaction times, although not so extremely short as in type B.

With an apparatus allowing such a form of movement the reaction time could not be understood without reference to a graphic record of the form of movement. Since with reactor M. this form of movement was perfectly natural, any apparatus which did not allow it to occur would have occasioned an artificial form of movement and turned the natural direction of the attention from a muscular preparation to react to an attempt to keep the contact on the key unbroken.

Let us turn now to the historical classifications of reactions established by Lange and his followers to see if we can classify reactor M. by any of these. Those reactions having short times with small mean variations, in which there is a muscular strain and frequent wrong or premature reactions, are classified as motor reactions. When, in addition, the reactor's introspection shows that his attention was on the movement to be made and not on the stimulus, we can be sure that the reaction is of Lange's motor type. Those reactions, on the other hand, having long times and large mean variations, with a definite sensory strain (that is, a strain in the sense organ rather than in the muscle) and no wrong or premature reactions, but among which at times, delayed reactions occur, are classified as sensory reactions. To these criteria would be added the introspection of the reactor that his attention had been on the expected stimulus and not on the movement to be made.

The description of reactor M. given above brings him clearly under the head of motor reactors. M.'s reaction times are very short, even below the 100-120σ determined by Lange as the average range of motor reaction time, and the mean variations are low, most of them below 15. Seven cases of reaction before the stimulus occurred scattered through five series. According to the introspections each of these cases was due to strained attention and extreme muscular preparation. Several special trials were made at unexpected times with a much longer interval between the warning signal and the stimulus. In each of these cases M. reacted before the stimulus, stating afterward that he "could not hold on any longer." In many cases, too, of the regular series in which a reaction time was recorded, both the shortness of the time and the sharp rise of the pressure curve before the stimulus was given, indicate clearly that these reactions were really begun before the stimulus occurred.

If we turn to M.'s introspections we find even clearer proofs of the motor type. Several times during the course of the investigation M. was asked to write at the close of a series his introspections on the direction of his attention. These statements did not change throughout the investigation: "I sit waiting passively for the warning signal with my attention turned in that direction. After the warning signal for perhaps one second I continue to sit passively. At about this point I begin to think it may be time to react and my muscles seem to tighten all over in preparation. My attention does not seem to be upon the stimulus which is coming nor upon my finger; it seems to be centered in a definite muscular preparation for a quick reaction. Although I do not pay any attention to the expected stimulus, yet if it is delayed I can only keep my finger on the key with difficulty. The reaction itself seems to be almost automatic. I make the necessary muscular preparations and the stimulus sets it off. Many times I am surprised to find myself reacting and to learn that the stimulus has occurred." At times M. stated that after the reactions he could recall that the stimulus had occurred. These statements tally, as we have seen, with the character of the pressure curves and the reaction times. Of the introspections recorded by M. after the individual reactions the most common type is "hard to hold my finger on the key" or "anxious to react." These two statements occur over 150 times out of 259 reactions.

M. then is clearly a motor reactor, and *naturally* so, since this type of reaction was assumed without any instructions. This brings him not only under Lange's classification but also under that of Baldwin's "natural types."

These facts of classification are of special interest since M. exhibited a definite *form* of reaction movement along with his natural motor type of reaction,—a decrease in pressure from the warning signal to the end of the record, the final movement in response to the stimulus being a simple sharp rise. This form of movement did not, further, develop by gradual practice but was, from the outset, related to the strength and direction of the attention and to the attendant varying degrees of muscular preparation. Any marked divergence from it, such as the double or partial antagonistic types, arose from an effort to check this normal tendency.

Some check experiments were tried with M. with explicit instructions on the direction of his attention. Forty-one reactions were had in which he was instructed to allow the movement to take care of itself and to turn his attention entirely on the expected stimulus. Thirty reactions were taken with instructions to concentrate the attention on the movement and not to pay any attention to the stimulus.

There was a marked difference in time between the two series. The average corrected time for the sensory series was 144σ while that of the motor series was 97σ. The mean variation for the motor series was 15 and that for the sensory 22. One reaction before the stimulus occurred in the motor but none in the sensory series. In the motor series M. was instructed to introspect on the *time* and on the direction of his attention and in the sensory only on the direction of his attention. The difference in the time of the two series and in the introspections shows that M. succeeded in establishing a corresponding difference in the type of attention. At the close of these series he stated that the motor series in every case appeared to be much more natural than the sensory, and seemed just like the earlier series.

The character of the movement in the motor series is identical with that in the earlier series. The pressure was steady in the period before the warning in 18/30 cases. There was a marked change in the direction of the pressure-curve at the warning in 28/30 cases. The pressure was decreased slightly or to a medium extent in the period after the warning in 20/30 cases, and to a marked extent towards the end of the record in 22/30 cases. There were no cases of increased pressure in this period. All but two of the final reaction movements were of the simple sharp kind, shown in type A, Fig. 8. There was one reaction of the gradual type and one reaction of the double type.

The summary of the sensory series shows the form of the movement in the critical parts of the record to be changed. In the period before the warning signal the cases are about equally divided between an increase and a decrease in pressure. There is a change in the direction of the pressure-curve at the warning signal in 38/41 cases. After the warning signal there are only 9/41 cases of a decrease in pressure, while 13/41 cases of an increase in pressure occur. This is in marked contrast to the motor series. In the period just preceding the stimulus there are only 5/41 cases of a decrease in pressure, while 15/41 cases of a slight increase in pressure occur. In these last two periods the cases of steady pressure are about equal in number to those of increased pressure. The reaction movement itself in the sensory series was the same simple sharp rise in 39/41 cases. There were two cases of gradual antagonistic reactions shown in type M., Fig. 8. The downward trend of the pressure-curve at the end is quite noticeable in these cases. These two antagonistic reactions were the effect of the stimulus rather than an effort on the part of the reactor to keep from reacting as in the "apparent" antagonistic reactions of M.'s regular series.

These two control series furnish further proof that the motor type of reaction was M.'s natural type, that a definite form of reaction movement accompanied it and that this form of movement changed with the changed direction of attention in the sensory reactions.

Fifty-one reactions were then taken with M. upon an electric break key. He at once experienced difficulty with this key in the motor reactions. His normal tendency to prepare for the final movement by lifting the finger could not be carried out, since the least movement of the finger upward broke the contact on the key. Although the attention was directed towards the finger and not to the stimulus, it was concerned with holding the finger down on the key rather than with the final upward movement. The average motor reaction time with this electric key was 120σ with a mean variation of 15. This is much longer than the average corrected time for the spring key.

M. stated after this series that he did not like the electric break key as well as the spring key. He felt "hampered, as though his finger were held down or tied down." He did not dare raise his finger at all or decrease the pressure on the key. After one series M. stated: "I find that more energy is consumed in pressing on this key than on the spring key. Time is required to overcome this pressure after the stimulus is given. The finger must be held down without any letting up." Not only was the attention turned to an effort to hold the finger down, but the introspections show that the attention was, at times, even drawn towards the stimulus, since it could not employ itself with a preparation to react.

A few reactions were taken with this same electric break key with instructions to attend to the stimulus. The average time was not increased in this case beyond that in the motor series with the same key. M. found much less difficulty with the electric key in the sensory series and stated that to him such a key seemed better suited to the sensory than to the motor type of reaction.

Such facts as these bring out even more clearly the conclusions already reached concerning M.'s definite natural type of reaction. They show also the marked effect which the different kinds of keys had upon the character of the attention. A key of the spring type appears much better suited to reaction work than the electric key since the latter may inhibit not only the natural tendencies of the reactor, making it impossible for him to react in a normal way, but also the directing of his attention in accordance with instructions.

Reactor H.

We will contrast with these results of reactor M. those of reactor H., also a student in the Graduate School. H. had had no previous training in reaction experiments of any kind, knew nothing of the distinction between the sensory and motor types of attention, was wholly unfamiliar with the apparatus at the outset, and was but little distracted by the new type of movement. Nothing was told H. as to the object of the investigation. He was told simply to lift the finger from the key on hearing the stimulus, and the movement employed by him, as observed by the operator, proved not to differ much from that of M., the reaction movement being a quick, upward jerk of the forearm from the elbow. The same suggestions concerning introspections were given H. that were given M. (see p. 75).

Fourteen series with a total of 280 reactions were secured, without additional instruction or knowledge of results. These series covered a period of eight weeks. The last nine series were taken in two consecutive weeks.

Three columns of times (see p. 72) are given in the quantitative summary of H.'s reactions, Table III. Except where expressly stated the "Corrected" time is the one utilized in the text. The "Movement Began" time is of interest only in indicating the amount of time which it is necessary to subtract for the antagonistic form of reaction movement. The time curves plotted in Fig. 10 show very clearly the relations between the different times. The difference between the upper line representing the chronoscope times and the middle line representing the corrected times, shows the error that had to be subtracted for the speed of the movement (see pp. 71-72). The error is a very constant factor with H. since the two curves correspond very closely. The average error for the entire series was 18σ. The difference between the corrected time curve and the lowest (broken) line representing the time to the beginning of movement, shows the amount of correction which it is necessary to allow for the antagonistic reaction. The average correction, i. e., the time consumed by the antagonistic phase of the movement, was 25σ. Since the amount of correction for the different antagonistic reactions is nearly the same, the difference between these two lower curves represents, roughly, the relative frequency of such

TABLE III.—*Summary of Average times and mean variations. Reactor H.*

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	2.2	173	36	154	32.2	130.8	23.2
II	2.4	176	20.5	172.5	14.8	134.3	22.4
III	2.0	159	20.7	142	18.	124.3	14.9
IV	1.9	147	18.2	138.7	16	104.7	16.7
V	2.0	158.6	22.9	144.8	13.6	128.8	15.1
VI	1.8	137.8	13.2	121.6	12	100.5	15.9
VII	1.8	138.	19.7	123.7	19.9	93.7	23.3
VIII	2.1	130.5	16.2	119	11.2	89	19.4
IX	2.2	131.6	13.4	121	13	93.6	16.7
X	2.2	127	14.2	114.2	14.7	101	12.7
XI	2.2	116.1	14.5	105.3	14.9	81.7	22.6
XII	2.1	133.1	16.7	117.8	16.2	101.6	13
XIII	2.1	131	18.3	124.2	18.2	96.2	16.8
XIV	2.1	120	20	114.7	20.9	81.9	29.1
Averages	2.2	148.4	18.8	129.5	16.8	104.4	18.5

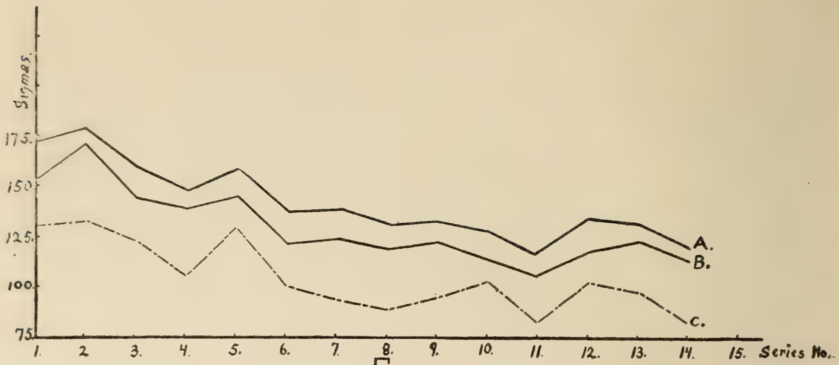


Fig. 10.

Reactor H.—Curves showing the relation between the length of the reaction times through the series.

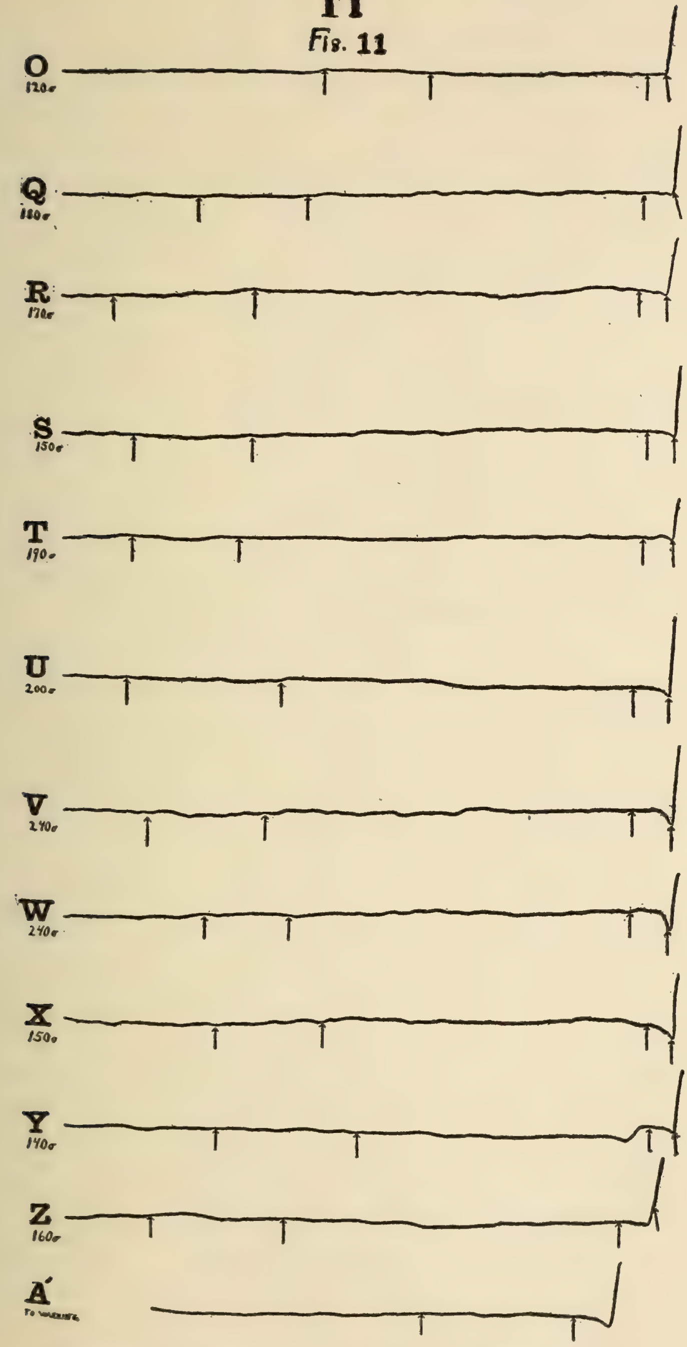
Curve A. represents the Chronoscope time; curve B. the Corrected time; and curve C. the time to the beginning of movement.

antagonistic movements. For example, there were fewer antagonistic reactions in series I, V, X and XII than in the other series.

The average time for all the series is fast, 129.5σ, and the mean variation is not large, 16.8. From series II through series XI there is a gradual decrease in time due to practice. The mean variation also decreases through these series. The average time increases again in series XII and XIII. In series XII H. had been excused from the middle of a long examination to keep his appointment for the reactions. The introspections show that he could not keep his mind from wandering to the subject of examination. In series XIII he stated that he was tired and nervous.

The qualitative summary, showing the characteristics of the pressure curves traced by H., is given in Table IV. H. is a more unsteady reactor than M. This unsteadiness is due to a nervous trembling of the hand, and appeared to a more marked degree when H. was tired. The most prominent characteristics in the curves are as follows: In the period before the warning signal there were 103/280 cases of increased pressure, the other cases being divided between a steady pressure, 95/280 cases, and waviness, 62/280 cases. The warning signal caused a change in the character of the pressure in more than one-half of the records, H

Fig. 11



Reactor H.—Pantograph reductions of pressure curves.

172/280 cases. In the period after the warning signal there were more cases of increased pressure (82/280) than of decreased pressure (15/280). The greatest number of cases in this period are steady and level (124/280). Towards the end of the record, before the final reaction, the proportions are not much changed,—76/280 cases of increased pressure; 40/280 cases of decreased pressure and 114/280 cases of level, steady pressure. The final reaction movement is manifestly of the antagonistic type, e. g., types S to W, Fig. 11. There are 195/280 such reactions. There are 68/280 reactions of the sharp type O, 4/280 cases of the gradual antagonistic type Z and 8/280 cases of the double type Y.

Such an account shows very clearly that reactor H. exhibited a definite form of reaction movement and one that he exhibited *naturally*. It differs radically from that shown by M. Was this form of movement developed through practice? The reaction *times* of H. show the effects of practice in the course of the series more definitely than did those obtained from M. As to the *form*, neither manifests any regular change or development due to practice; both show their characteristic variations in form in the early as well as in the late parts of their total series. But whereas M.'s final reaction movements were either sharp or gradual in form, with few antagonistic, H.'s were either sharp or antagonistic, the latter greatly preponderating. Fig. 11, types O to W, examples selected (not, however, in consecutive order) from all the series, shows his characteristic variations in form. The forms which appear most commonly in H.'s records are those of types S and U (183/280 cases). These forms have been classed as "slight antagonistic reactions."

Comparison of the different forms of movement with the character of the attention as shown by the introspections, again brings out the point that there is no development of a preponderating reaction form with practice. There are, for instance, more sharp than antagonistic reactions in series I, X and XII. In series I, H. was learning to use the machine and his attention was much more on the movement and finger than on the stimulus. In series X he shows a marked tendency to direct his attention

TABLE IV.—*Qualitative summary. Reactor H.*

Key *Series No.*	*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*XI*	*XII*	*XIII*	*XIV*	*Total*	
Rise before warning	2	1	2		1	2	1	1		1	3	3	1	1	19	
Fall before warning	7	6	7	9	10	6	7	8	14	6	6	7	3	7	103	
Level before warning	8	7	6	7	2	10	6	9	4	9	7	4	7	9	95	
Wavy before warning	4	6	5	4	7	2	6	2	2	4	3	5	9	3	62	
Change at warning	15	14	9	12	16	10	16	15	11	10	14	15	12	3	172	
No change at warning	6	5	10	8	3	10	4	5	9	10	6	4	8	17	105	
Reacted to warning		1	1		1										3	
Marked rise after warning															0	15
Medium rise after warning												2			2	
Slight rise after warning	1	1		1		1	2	2		3	1		1		13	
Marked fall after warning															0	82
Medium fall after warning			1	3	2	2			3	1	2		3		17	
Slight fall after warning	5	4	9	3	5	9	7	6	4	4	1	1	1	6	65	
Level after warning	9	8	5	12	9	5	6	10	12	11	13	4		10	124	
Wavy after warning	6	7	4	2	4	3	5	3	2	2	2	12	15	4	71	
Marked rise towards end							2								2	40
Medium rise towards end		2		3	1		1	1			4	2			14	
Slight rise towards end	2	2	1	1	2		1	2	3	5	1	2		2	24	
Marked fall towards end								1			2	1			4	76
Medium fall towards end	2	2	2	2	2		2	2	2	1	4	1	1	6	29	
Slight fall towards end		1	4		4	5	9	5	5	3	2	3		2	43	
Level at end	17	12		14	7	14	5	8	8	10	6	5	2	6	114	
Wavy at end			11		2	1		1			1	6	17	4	43	
Reacted before stimulus										1		1		2	4	
Sharp reaction	11	2	4	1	7	4	8	1	2	12	4	9	3		68	
Gradual reaction														1	1	
Slight antagonistic reaction	9	15	15	19	11	16	9	18	15	8	13	8	12	15	183	199
Marked antagonistic reaction	1	1	1								2		4	3	12	
Gradual antagonistic reaction		2					1					1			4	
Double reaction					1		2	1	1			1	1	1	8	
Number of reactions in series	21	20	20	20	20	20	20	20	20	20	20	19	20	20	280	

to the movement rather than to the stimulus. In series XII, H. was tired and distracted by an examination. Like reactor M., therefore, H. assumed at the start a very definite form of reaction movement and maintained it in the majority of reactions to the end of the fourteen series, departures from it being due, introspectively, to changes in the direction and amount of the attention.

The sharp form of final reaction movement is accompanied by two types of introspection. Either, "fast, afraid of reacting too soon," or "medium slow, wavering attention." These two types of introspection indicate that the sharp reactions occurred when the reactor's attention was turned towards the finger or movement, or was generally lowered. The slight antagonistic reactions occurred when the conditions were the most normal. At such times H. stated that his attention was good and turned toward the expected stimulus. The three most marked antagonistic forms, on the other hand, occurred when the reactor's attention was distracted by some other thoughts. Examples of this are found in series III, No. 8: "Bad attention, thinking of my failure to get here yesterday"; series XI, No. 16: "Slow, thinking of preceding reactions"; series XIII, No. 2: "Signal came before I was ready." The four gradual antagonistic reactions also occurred at times of distraction. Two were reactions before the stimulus. In the others H. states, "Mind on warning; hard to hear warning." Examples of these reactions are shown in Fig. 11, types Z and A′. The double reactions, as in the case of M., were caused by the attempt on the part of the reactor to check a reaction before the stimulus. In both cases H. was conscious of this form of movement since he stated after each, "Started and stopped." An example of the double reaction is shown in Fig. 11, type Y.

Such comparisons show more emphatically a definite relation between the form of movement and the character of the attention. Marked departures from the normal type of attention were accompanied by marked departures from the normal form of movement. The same relation holds true in the period just before the final movement made in response to the stimulus.

Only two cases of a marked rise towards the end appear. These two are followed by the double reaction movements which we have just examined.

The fourteen cases of a medium rise towards the end are accompanied by introspections which show distraction or a lowered attention. For example, series XI, No. 17: "Simply could not concentrate my attention"; series XII, No. 15: "Poor attention, thinking of examination."

Another characteristic appears in H.'s data showing him in marked contrast to M. In the period before the warning, H. exhibits a strong tendency to increase the pressure. The warning signal often emphasizes this. The increase is interesting in the light of a statement made by H. at a later time, which will also be used in another connection. He said that he felt the need of a firm foundation from which to start the reaction movement,—the increase in pressure seemed to be an attempt to "strike bottom." M., whose pressure lessened, apparently felt no such need. In general, too, the warning signal introduced less change in the character of H.'s pressure than in that of M.

Where does reactor H. belong judged by the criteria for sensory and motor reactions? His reaction times are much longer than those of M. (60σ longer), but they are still short compared with the 200-220σ established by Lange for sensory reactions. The mean variations are also somewhat larger than those of M. In themselves the reaction times and the mean variations would not definitely classify H. as either sensory or motor. Considered in conjunction with the almost complete absence of premature reactions they offer presumptive evidence, however, that he was of the sensory type. Only four reactions before the stimulus occur. The introspections show that one of these occurred when the reactor's mind was wandering, and the other three when the attention was turned away from the stimulus towards the finger. Several trials were made to test the effect of long warning intervals given at unexpected times. These reactions were not included in the normal results and occurred at the end of the different series. In no case did H. react before the stimulus after one of these long intervals. He stated that his muscular preparation for the reaction movement increased, but that his attention turned more and more intently towards the delayed stimulus.

His further introspections give clear proof that H. was of the sensory type. He made the following general statement in regard to his attitude and attention during the majority of reactions: "I sit waiting for the warning signal with my attention wholly in that direction. After the warning comes I seem to tighten up my muscles all over in preparation for the coming reaction and my attention turns towards the stimulus, waiting for it to sound. Sometimes the increased tightening of the muscles draws my attention towards my finger, causing my attention to fluctuate back and forth between my finger and the sound. I always hear the stimulus, however, and would not react until it sounds." Only five times out of the 280 does there appear any introspection that would indicate attention turned toward the finger or the movement. There are 25 introspections which record lowered attention or distraction. The remainder (250) state that the attention was "good" or "normal," i. e., on the expected stimulus.

These introspections clearly place H. among the sensory reactors; and like reactor M., H. assumed his type of reaction naturally without any instructions, thus bringing him also under Baldwin's classification of natural types. This fact is of particular interest because, with this natural type of attention and reaction went also a definite form of reaction movement,—both entirely different from the natural type of attention and the form of movement assumed by M. Whereas M. decreased the pressure from the warning signal to the end of the record and responded to the stimulus by a simple sharp rise, H. maintained a constant pressure or increased the pressure from the warning signal on and responded to the stimulus, in the majority of cases, with an antagonistic reaction. Any marked changes, furthermore, in the form of movement could be correlated with changes in attention, as in the case of M.

This antagonistic form of movement has a marked effect on the length of the total reaction time. As we have seen, it would be necessary to subtract 25σ from the reaction time of H. to obtain the time between stimulus and the real beginning of the reaction movement, i. e. the beginning of the antagonistic phase. So far as we have gone it would not be possible for us to say, however, that the antagonistic form of movement would *always* occur with the sensory type of attention, and the simple sharp form with the motor type. These forms might be purely individual. This point needs further investigation and a discussion of the facts will be postponed until the results of other reactors have been described.

Special Series.—Three series of reactions (thirty in all) were taken in which H. was directed to center all his attention on the coming stimulus and to let the movement take care of itself. The average time is 147.1σ. This is 17.6σ longer than the average time of the regular series. The mean variation is lower than that of the regular series by 3.4. The average reaction time brings H. nearer the traditional sensory type. H. stated that this form of reaction seemed natural to him, and that he had not thought of his finger at all, or of the movement, in the last two of these series. In the pressure curves the same characteristics appear (but in greater degree), which appeared in those of the regular series. As in the regular series the cases are about equally divided between a downward pressure before the warning and a steady pressure. There are relatively more cases of change in pressure at the warning. There are only two cases of a rise after the warning, neither being of the marked type. There is a much greater proportion of increases in pressure after the warning, 15/30, than in the regular series. The remaining cases are level or wavy. There are only two cases of a rise towards the end, neither of them marked, but still more cases, relatively, of increase in pressure, 20/30, than in the regular series. The final reaction movement in almost every case is of the antagonistic type, 25/30. Eighteen of these are of the slight antagonistic type, shown in Fig. 11, types S, T and U, and the remaining seven of the marked types, V and W. There is only one double reaction, which occurred when the attention shifted slightly to the movement. In the series in which the attention of the reactor was most strongly centered on the coming stimulus, more antagonistic reactions of the marked type than of the slight type appear. At the end of this series the reactor stated, "Absolutely no attention paid to finger." We seem justified in the assumption that this series is of a more uniform sensory type. In the regular series this uniformity was interfered with by fluctuating attention. By distributing the forms of movement in the first *regular* series under the types of attention in which these movements occurred, we found the form of movement most regularly accompanying the sensory type of attention to be a downward pressure preceding the reaction, followed by the antagonistic movement in the reaction itself. In this special sensory series we find these characteristics more marked.

Three series of motor type were attempted in connection with these special sensory series. The reactor did not succeed in establishing the motor type of attention. He stated that he simply "fixed his attention upon his finger and waited for the stimulus," which seemed rather a paradox. The records show an increase in time of 3σ over that of the special sensory series. This fact in itself, aside from the introspections, indicates that there was very little difference in the character of the two series. The mean variation is very much increased, 21.3, showing that the attempt to fix the attention for a motor series proved a distraction. We find, in fact, the pressure characteristics of the sensory series appearing in a more marked degree. There are no cases of rise after the warning or towards the end and 21/28 cases of a fall towards the end. All but one (27/28) of the reactions are of the antagonistic type. These are distributed as follows: 21/27 slight antagonistic reactions, 5/27 marked antagonistic reactions, 1/27 gradual antagonistic reactions.

H. was carefully questioned as to his attitude in these reactions. It was found that he did not interpret "attention on the movement" or "attention on the finger" to mean a preparation for a quick upward movement. He fastened his attention on the finger and was more concerned than ever in holding it down upon the key. A good example of this, in a single reaction, is as follows: the reactor stated, "Medium time; attention on finger." The time was really very slow, 225σ. As to the form of the movement, there is a very marked fall in the line after the warning, meaning a marked increase in the pressure. This is followed by a very marked antagonistic reaction. H. stated, furthermore, that he always waited for the stimulus in these series. No cases of reaction before the stimulus occur. A special trial was made at the end of one series to see if the reactor could be made to react before the stimulus. The interval between the warning and the stimulus was increased to 3.2 seconds. H., however, reacted in normal time (140σ) and did not seem to be at all disturbed by the delay. These attempted motor series seem, then, to be sensory, with the additional complication of increased attention to holding the finger on the key. H.'s whole attitude was so strongly sensory that a very long practice series would have been necessary to develop the motor type of attention. These series then, do not serve as a check on the sensory series. They show, however, how strong the tendency in one direction can become and how difficult it would be ever to get a motor series with such a reactor. This explains very well what the Leipzig experimenters call absence of "Anlage."

One series of discrimination reactions was taken with H. One electric hammer was placed at the right of the reactor, with a second at his left, both behind the screen. By means of a multiple switch either of these hammers could be thrown into the reaction circuit. H. was instructed to react to the sound on his right and not to react to the sound on the left. The average time was 247.1σ. The mean variation was large, 51.3. The form of movement corresponds to that made in the sensory series. Of the fourteen reactions to the stimulus on the right, ten show a change at the warning; ten increased pressure after the warning; ten show a strongly increased pressure towards the end. There are no cases of a decrease in pressure during the period between warning and stimulus. The final reaction movement is of the antagonistic type in all but two of the reactions. These antagonistic reactions are more marked than any of the forms shown in the earlier series; the sharp downward trend of the curve often beginning, too, before the warning signal. There is one double reaction, the first reaction taken. The reason for this is evident from the introspections. The reactor managed to see the spring on the electric hammer move before the sound came. This caused the preliminary start which was checked until the stimulus sounded. The hammer was then moved completely behind the curtain and this distraction did not occur again. The character of the pressure when the stimulus at the left sounded is very interesting. There is the same gradual increase in the pressure up to the point at which the stimulus occurs. Here the pressure becomes still greater, causing a trough in the curve about 200σ in length, after which the pressure is steady, or in some cases decreased until the motor is stopped. This involuntary downward start was noticed, when it became marked, by the reactor and was recorded by him in the introspections.

The data thus far seem to show, then, that when the attention is concerned with the coming stimulus, there occurs an increase in pressure during the period preceding the reaction (reactor H.). When the attention is concerned with the movement to be made (reactor M.), there occurs a decrease in pressure during the same period. We will leave the discussion of reasons until later.

A series of twenty reactions was taken with H. on a regular electric break key. He was allowed to react in his normal way. The time was increased to 152.1σ. The mean variation was lower than the average for the regular series, 15.7. The reactor said after his first trial, "This feels more like it." At the end of the series he stated that this key seemed to require more pressure than the spring key. This fact was a recommendation in itself to him. He felt, as already stated, the need of a firm foundation from which to start. The spring key did not furnish it. He believed that all his times were much faster with the electric break key than with the spring. H. was then tested with a large tambour. The times were increased still more. The average for the series of 20 reactions was 155.9σ. The mean variation was also increased, 24.1. The form of the movement was the same as with the spring. There was a marked downward pressure before the stimulus and a more marked antagonistic reaction movement. H. did not like this type of apparatus at all; it seemed like "trying to start to run from a bed of sand." The rubber of the tambour did not give him even as solid a basis as the spring.

These statements in regard to the different forms of reaction apparatus are interesting in view of the antagonistic tendency exhibited by this reactor. Whereas M. found the spring key most suited to the preparatory rise in the period preceding the reaction movement, H. found that the electric key gave a greater feeling of security in view of his tendency to press down in the period preceding the reaction so as to get a solid basis from which to start the upward movement. The electric key, however, increased the reaction time, doubtless because the rigidity of such a key did not allow free play of the antagonistic tendency but rather interfered with it, acting not only as a check on the antagonistic movement but upon the subsequent rise. The fact that this key felt most secure doubtless accounts for the feeling of the reactor that he was making faster reactions.

After all the test experiments with spring, tambour, and key, H. was told about the antagonistic tendency and asked if he had noticed it and if he knew of any other facts in his experience which would account for it and for his sensory type of attention. He said he had noticed this final downward push only a few times. This accords with the more detailed introspections, where we find only three such statements in the regular series. He stated that such a movement seemed to accord with his general nervous habit of suddenly tightening any muscles he was about to use before the actual movement was made. In his undergraduate days he had been a hundred-yard-dash man. So far as he could remember he had never "jumped the gun." He always gave a preliminary dig with his feet and crouched slightly as if to spring, the instant before the signal came. He said that this preliminary "start" had often caused the men at his side to leave their marks before the signal but that he had never done so himself.

Summary.—We have shown:

First, that the type of pressure up to the stimulus, and the type of the final reaction movement shown by reactor H. in the regular series appear more often and to a more marked degree in the special sensory series.

Second, that it was not possible for reactor H. to establish the motor type of attention in the three series tried.

Third, that whenever the attention was directed away from a fast final movement, the pressure in the period preceding the reaction movement tended to increase.

Fourth, that there is some reason to believe, in view of these facts, that the antagonistic movement appears more often in the sensory type of reaction (reactor H. compared with M.).

Fifth, that the form of key has a marked influence on the reaction time.

REACTOR E.

Having dealt thus fully with the two previous reactors, in order to show the methods of procedure and of analysis of results, it will not be necessary to go into such detail with the remaining reactors.

Both M. and H. exhibited definite forms of movement which could be correlated with the reaction times. But more than this, these reaction times and forms of movement accompanied definite types of attention, utilized by earlier investigators in their classification of reaction types. With E. we come to a reactor whose movement records did not show uniform characteristics throughout and whose average reaction times were scattered from 244σ to 105σ. At first sight it would seem impossible to harmonize such results with those of the previous reactors. A careful study showed this, however, to be possible.

E. had had no training in reaction experiments; the ordinary instructions to raise the finger on hearing the stimulus were given him at the beginning of the series. A total of 202 reactions was taken in these uninstructed series. The average corrected time for the ten series (Table V) is 155.9σ with a mean variation of 30. The average corrected time of series I is long-244σ with a M. V. of 50. The times decrease regularly and the M. V.'s fairly regularly to the ninth series (time 105σ; M. V. 24). The time goes up again slightly in the tenth to 110σ with a M. V. of 18.

TABLE V.—*Summary of average times and mean variations. Reactor E.*

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	2.3	263	53	244	50	238	50.7
II	1.6	227.5	59.5	204	54.8	198	51.4
III	2.2	211	21.6	190	29.3	178	26.3
IV	2.3	185	32	175	29.2	153.2	29
V	2.3	185.9	33.4	156.7	28.5	149.7	21.2
VI	2.2	166	28.4	141.9	34.1	118.1	38.9
VII	2.2	142.5	19.6	120.6	17.6	119.6	17.9
VIII	2.1	137	20.2	111.5	22.2	110.8	21.4
IX	2.0	137	20.1	105.3	24.2	96.4	29.7
X	2.3	139.5	15.6	110.7	18.1	105.2	25
Averages .	2.15	179.34	30.34	155.97	30.79	147.7	31.15

Such a regular decrease in the average reaction times and mean variations indicates that we are dealing with a marked practice series. In order to show this more clearly, the times were distributed according to their frequency (Fig. 12, curve A).

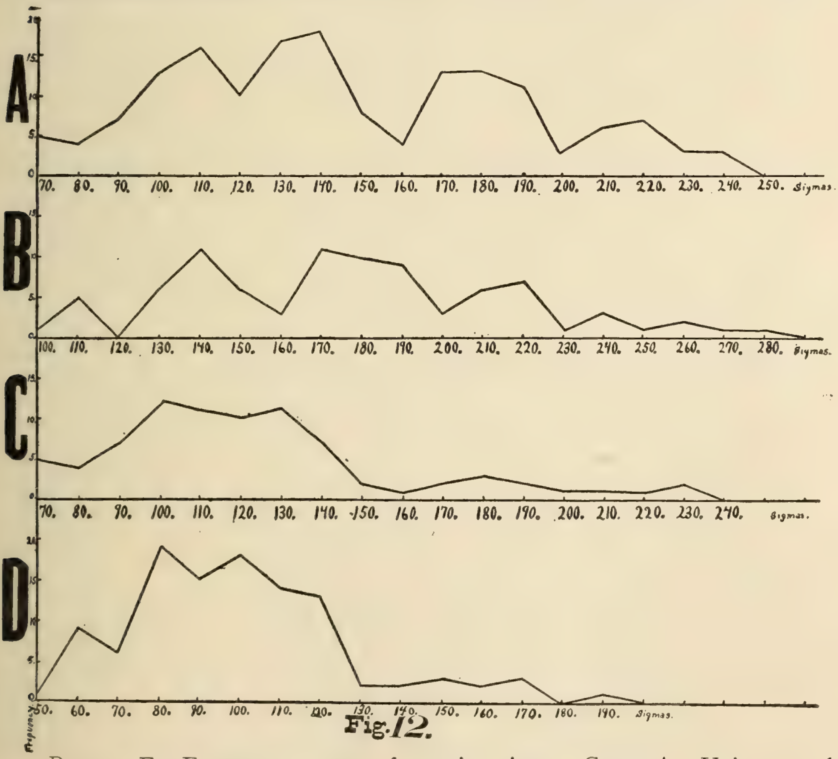


Fig. 12.

Reactor E.—Frequency curves of reaction times. Curve A. Uninstructed series. Curve B. First half of uninstructed series. Curve C. second half of uninstructed series. Curve D. motor series.

TABLE VI.—*Qualitative data sheet. Reactor E.*

Key — *Series No*	*Uninstructed Series*											*Total*	*Motor Series*						*Total*	*Total*
	I	*II*	*III*	*IV*	*V*	*Total I-V*	*VI*	*VII*	*VIII*	*IX*	*X*	*VI-X*	*XI*	*XII*	*XIII*	*XIV*	*XV*	*XVI*	*XI-XVI*	*I-X*
Rise before warning.....	1	1	1		2	5			1	4	3	8	2	2	1				5	13
Fall before warning......	1	5		6	2	14		3	3	4	2	12	1		8				9	26
Level before warning	14	10	9	9	14	56	19	9	14	10	13	65	10	8	9	2	9	4	42	121
Wavy before warning....	4	7	10	4	2	27	1	8	2	2	2	15	7	3	2	19	11	16	58	42
Change at warning.......	17	19	16	15	13	80	14	11	13	14	18	70	16	12	17	19	15	13	92	150
No change at warning....	2	3	4	4	6	19	6	9	7	6	2	30	4	1	3	2	4	7	21	49
Reacted to warning	1					1						0							0	1
Marked rise after warning	1	1				2		1				1							0	3
Medium rise after warning	5	7	2		1	15 } 40		1		2	6	9 } 40	2		5		3		10 } 34	24 } 80
Slight rise after warning..	9	6	3	2	3	23	6	7	3	6	8	30	10	6	4		4		24	53
Marked fall after warning						0						0							0	0
Medium fall after warning				4		4 } 8						0 } 7							0 } 2	4 } 15
Slight fall after warning..				3	1	4	3		2		2	7		1	1				2	11
Level after warning......	2	4	4	8	7	25	10	10	5	7		32	1	4	3		4	1	13	57
Wavy after warning......	2	4	11	2	8	27	1	1	10	5	4	21	7	2	7	21	9	19	65	48
Marked rise towards end	1				3	4		1	1	5	2	9			1	4		1	6	13
Medium rise towards end	7	5	4	1	3	20 } 47	2	6	3		5	16 } 44	1		6	3	1	3	14 } 43	36 } 91
Slight rise towards end..	3	6	4	3	7	23	4	3	6	2	4	19	5	1	6		7	4	23	42
Marked fall towards end..		1		1		2				1		1				5		1	6	3
Medium fall towards end	2	2	2	1	1	8 } 21	2		2	2		6 } 19	2	2	1	2	1	2	10 } 22	14 } 40
Slight fall towards end...		2		9		11	3	5		2	2	12	1	1	1		1	2	6	23
Level towards end.......	6	5	5	3	6	25	9	5	8	7	5	34	6	6	4	2	5	7	30	59
Wavy towards end.......			5			5					2	2	5	3	1	4	5		18	7
Reacted before stimulus..						0						0			1	2	1		4	0
Sharp reaction	6	11	3	4	9	33	8	13	19	12	18	70	16	6	15	7	18	16	78	103
Gradual reaction	10	5	11	1	10	37	6	6		3	1	16	3	7	5	11	1	4	31	53
Slight antagonistic reaction		4	1	8	1	14	4	1	1	2		8				1			1	22
Marked antagonistic reaction	3	2	5	4		14 } 29	1					1 } 12							0 } 2	15 } 41
Gradual antagonistic reaction				1		1				2	1	3				1			1	4
Double reaction..........						0	1			1		2	1			1	1		3	2
Reactions in series......	**20**	**23**	**20**	**19**	**20**	**102**	20	20	20	20	20	100	20	13	20	20	20	20	114	202

This is roughly a double frequency curve. The most frequent of the shorter times lie between 110σ and 170σ, and of the longer times between 170σ and 190σ. So much for the unintructed series as a whole.

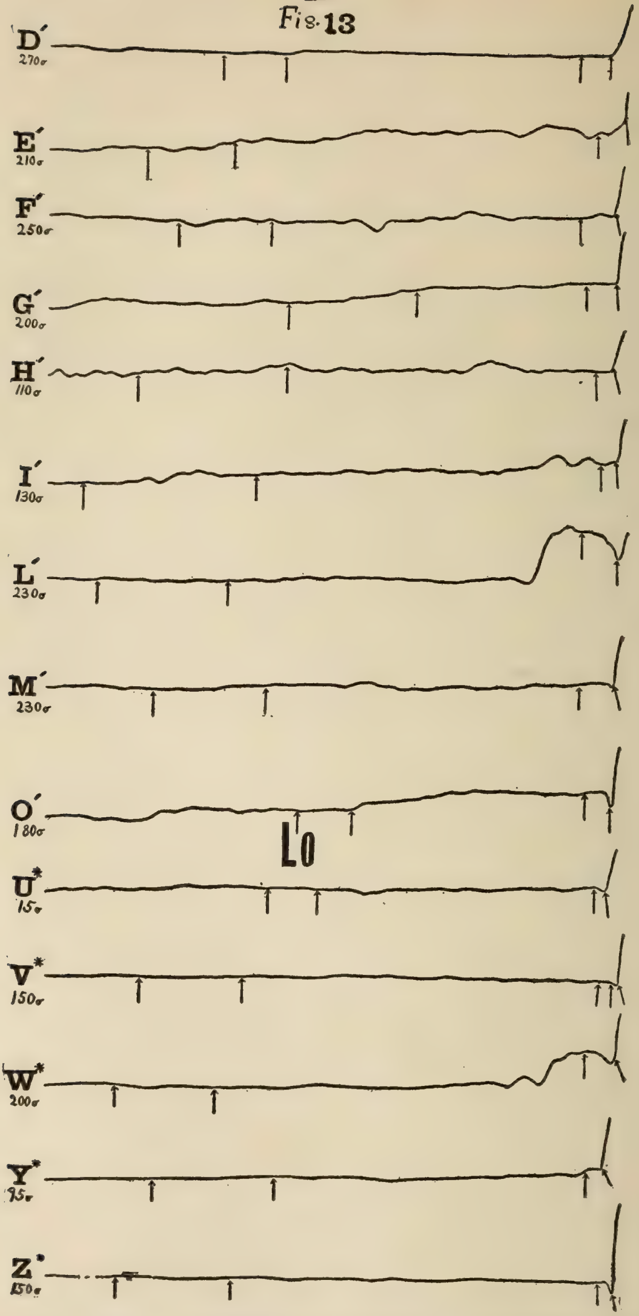
Dividing it into two parts, series I to V, and VI to X, and distributing the reaction times in these two half series according to their frequency, we get curve B, Fig. 12, for series I to V, and curve C for series VI to X. Curve B shows a very wide distribution of times with no grouping in any one region. Curve C gives a marked grouping of the times in the region between 100σ and 130σ. These curves, together with the general decrease of the average times, show clearly that reactor E assumed the short reaction times in the course of practice.

Reactor E.'s pressure curve (Fig. 13) is much more irregular than any thus far considered. Such irregular records as those shown in types E′, F′ or I′ are very typical. There are very few records of type H′, where the smaller waves due to hand trembling occur, and these are almost entirely in the last two series, in which the reactor was tired and nervous. The large inequalities we found to be accounted for by the shoulder and arm movements of the reactor.

In Table VI a summary is given of the characteristic parts of the pressure curves for each half separately of the total uninstructed series. We find in both halves a very remarkable agreement in the characteristics of the curves up to the final reaction movement. The most prominent are as follows: The pressure is steady to the warning signal. When the warning occurs there is usually a marked rise in the curve. This amounts in some cases almost to a partial reaction to the warning. Very seldom does the warning cause an increase in pressure. There is usually a decrease in pressure in the period after the warning signal, with fewer cases of a steady pressure, and very few of increased pressure. There are many cases of decrease in the pressure towards the end, in preparation for the reaction, more of the marked decreases occurring in the second than in the first half of the ten series. There are relatively few cases, towards the end, of increase in the pressure with somewhat more of a

E.

Fig. 13



Reactors E, and Lo.—Pantograph reductions of the pressure curves.

constant pressure. There are many cases of waviness in both the period before the warning and the period immediately after the warning, and but few cases of waviness at the end.

When we turn to the final reaction movement there is a marked change in type between the two halves of this uninstructed series. In the first half the reactions are almost evenly divided between those of the sharp type G′, Fig. 13 (32/102); the gradual type D′ (37/102); and the antagonistic types M′ and O′ (29/102). There are no reactions of the double type. In the second half of the series, 70/100 are of the sharp type G′; 16/100 of the gradual type D′; 12/100 are antagonistic in type (M′ and O′), and 2/100 are of the double type L′.

We see, then, that a change in the form of the final reaction movement accompanies the change from longer to shorter average times. With the longer average times occurred relatively more antagonistic and fewer sharp reactions, while with the shorter times, almost all reactions were of the sharp, and very few of the antagonistic type. This agrees with our conclusions in regard to the earlier reactors.

What was the character of E.'s attention throughout this uninstructed series? The frequency curves B and C, Fig. 12, have shown that E. developed the short reaction times and low mean variations with practice. No wrong or premature reactions occurred in either half of the series. Nothing was said to the reactor about the distinction between the sensory and motor types of attention. At the end of series IV, he stated of his own accord that he thought his attention had been on the signal. At the end of series V, he said his attention seemed to have been divided very equally between the signal and the finger. At the close of series VII, he stated, "It seemed to me that the times were faster than usual." Such a statement appears to indicate greater interest in the speed of the movement. In series IX, the reactor states, for reactions Nos. 3 and 4, "Slow; thinking of signal." Such a statement indicates that this form of attention was then a distraction. On the other hand there are many introspections in regard to the movement of the hand or arm which show clearly the direction of the attention at least during a portion of the reaction period. For example, in series IX, reaction No. 6, the reactor says, "Medium fast; marked downward pressure." We find that this is a correct introspection of the movement. Or again, in the same series, reaction No. 9, he states, "Medium fast; pressure in jerks."

In almost every case in series VII to IX, E. makes some statement as to the character of the pressure, such as "steady," "unsteady," "wavy pressure at end," etc. Such statements leave little room for doubt, when taken in connection with the times, that E. was a reactor of the motor type by the time series VII or VIII had been reached. At the beginning of series X, E. was told of the different types of attention and asked to introspect on this point carefully. This he did and at the end stated that he thought his attention went more naturally to the movement than to the stimulus. Such a statement taken by itself might prove of little value, since the effort to notice the character of the attention might have produced an abnormal attitude. It furnishes added proof of the motor type of attention, however, when taken with the other factors.

These introspections, with the frequency curves, show clearly that E. started with a sensory type of attention and developed the motor type through practice. The forms of the final reaction movements in the first half of the uninstructed series I-V, more nearly correspond, furthermore, to those found with the sensory type of attention in the case of reactor H. The forms of final movement in the last half of the series, VI-X, correspond to those found in the motor series of reactor M.

Those phases of the curves *preceding the final reaction movement* did not, however, exhibit unambiguously similar correspondences to the curves of M. and H. With them we found that a rise after the warning and towards the end occurred in the motor type of attention, and that a fall after the warning and towards the end was more often associated with the sensory type of attention. With reactor E. we have seen, however, that there is little obvious change in the character of the pressure in these parts of the record, as the reaction changes from the sensory to the motor type. The reason for this can be seen by an inspection of the curves themselves. The curves are so irregular and there are so many waves, both of the small hand waves and the large arm waves, that these tend to conceal any definite tendency to decrease or increase the pressure.

The cases of observed increase in pressure after the warning were therefore distributed according to the character of the attention stated in the introspections, in order to get, more in detail, the correlations between movement-form and attention. In the first half of the uninstructed series, eight cases of increased pressure after the warning occur, seven of these being in series IV. At the end of this fourth series E. stated of his own accord that his attention had been on the stimulus. In the second half of the uninstructed series only seven cases of increase in pressure, very slight, occur, after the warning. The records show that these are due to chance arm movements rather than to a definite tendency to increase the pressure in this period. In a motor series (XI-XVI, to be considered later) only two cases of a slight increase of pressure after the warning are found. These also are due to the larger chance arm movements. Of the increased pressures towards the end, in the first half of the uninstructed series, over half occur in series IV, spoken of by E. as accompanying attention to stimulus. All but one of the remaining occur in the first three series before a definite type of attention had been developed. In the second half of the series there is only one case of a marked increase in pressure towards the end. This is in series IX, and preceded a gradual antagonistic reaction, noticed by the reactor, since he states in the introspections, "Marked downward pressure." There are six cases of a medium increase in pressure towards the end in the second half of the series. These are due to unsteady pressure rather than to a definite increase.

Although, then, E.'s curves for the periods preceding the final reaction movement do not vary much on the average as his type of reaction develops from sensory to motor, it appears that specific cases of increased pressure may be, in general, correlated with a non-motor type of attention. Coming now to the final reaction movement itself which, as we have seen, did markedly change with the advent of the motor type, the number of sharp reactions increasing and that of antagonistic reactions decreasing, we get the following more detailed analysis. Fourteen *slight* antagonistic reactions occur in the first half of the uninstructed series. Four of these occur in series II, one in series III, eight in series IV (which we have seen was regarded by E. as accompanying attention to the stimulus), and one in series V. Fourteen *marked* antagonistic reactions occur, furthermore, in this first half of the series, distributed as follows: Three in series I, two in series II, five in series III, and four in series IV. There are no marked antagonistic reactions in series V. One of the two in series II is interesting in relation to the introspection. The time was very long, 355σ. The reactor realized this and made a great effort to react quickly. This increased effort resulted in the final antagonistic movement.

We have seen that the attention had not reached the motor type in this first half of the uninstructed series. In the second half eight slight antagonistic reactions occur, distributed as follows: four in series VI, one in series VII, one in series VIII and two in series IX. Of these slight antagonistic reactions, two occurred when E. was distracted by an outside influence. In one case E. records, "Making a greater effort." In another case he states, "Thinking of signal; slow." This was in series IX. The only other slight antagonistic reaction in series IX occurred because of an attempt to check a reaction before the stimulus. Only one marked antagonistic reaction occurred in the second half of the uninstructed series. This was in series VI. A man entered the room just as this reaction occurred, causing the reactor to give an involuntary start. E. stated that this did not cause a distraction, but the start shows in a most marked antagonistic reaction. As E.'s reaction type becomes definitely motor, the antagonistic reactions not only, then, become fewer, but are manifestly correlated with exceptional states of attention.

Two double reactions occur, furthermore, in the second half of the series, with none in the first half. A double reaction, as we have found with reactors M. and H., is associated with a strong tendency, of which the reactor is aware, to react before the stimulus. This appears in the motor type of attention. The records show, finally, that in the first half of the series more than one-third of the reactions were of the gradual type. In the second half of this series the proportion of gradual reactions had dropped to less than one-sixth of the total, being replaced by sharp reactions.

Since E. seemed to have reached the limit of speed and since he had adopted a more or less definite type of attention, he was told at the end of series X to center his attention, in the remaining series, on the movement to be made. The next six series (XI-XVI) will be referred to, therefore, as the "motor series." At the outset the conscious effort to concentrate the attention in this way proved a slight distraction to the reactor, since the times and mean variations were temporarily raised. This distraction was only temporary, however, the effort not extending beyond series XI.

The average time for this motor series was 101σ, the mean variation 20 (Table VII). The time decreased regularly from 123σ in series XI to 83σ in series XIV. It increased slightly in series XV (98σ), since the reactor was tired and nervous, stating, "I found it hard to concentrate my attention during this series; all the times seemed longer than usual." The time of series XVI (94σ) offered, however, no similar introspective reasons. Distributing the times in these motor series according to their frequency, we have curve D, Fig. 12. This curve shows the motor type of reaction to a more marked degree than in the original uninstructed series. The most frequent times lie between 80σ and 120σ. This not only adds force to the earlier statement that the motor type of attention had been assumed in the later uninstructed series; it shows also the ease with which the reactor was able to carry out the instructions as to the motor type of attention. Two reactions before the stimulus occur, both due to anticipation of the signal by the reactor.

The same characteristics of form, found in the last half of the uninstructed series, appear in the motor series in a more marked degree (Table VI). There is little difference, however, in the character of pressure in the periods preceding the reaction, except that in the motor series there are but 2/114 cases of increased

TABLE VII.—*Summary of averages and mean variations. Motor series. Reactor E.*

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
XI	2.2	149	23.3	123.5	21.8	118.5	24.7
XII	2.2	136	17.39	112.8	13.5	112.8	13.5
XIII	2.0	121.8	24.2	96.4	20.8	96.4	20.8
XIV	1.7	115	28.1	83.4	28.5	78.9	32
XV	2.2	132.6	24.1	97.8	17.4	97.8	17.4
XVI	2.2	119.2	19.2	94.1	19.8	94.1	19.8
Averages	2.0	128.9	22.7	101.1	20.3	99.7	21.3

pressure after warning as against 7/100 cases in the former series. It is in the final reaction movement itself that the differences between the two series are most marked. Whereas the sharp type of reaction is characteristic to an equal degree of both, the number of gradual reactions in the motor series has increased to 31/114 over the 16/100 in the last half of the uninstructed series; and the number of antagonistic reactions has fallen from 12/100 in the latter to 2/114 in the motor series. Neither of these is marked. Both, furthermore, appeared introspectively as exceptions. For one E. stated, "unsteady pressure," the form of reaction doubtless resulting from an attempt to control the unsteadiness. In the other antagonistic reaction the attention wandered. Finally, for each of the three double reactions occurring in the motor series, E. stated, "Reacted too soon." Premature reactions are peculiarly characteristic of motor attention.

Summary. 1. E. developed the motor type of attention and progressively shorter reaction times in the course of a practice series.

2. With this change in attention went also a change in the form of the reaction movement.

3. The most marked feature of the change was the decrease in the number of antagonistic reactions and the corresponding increase in the number of sharp reactions.

4. In the motor series which followed the practice series there was a still greater decrease in the number of antagonistic reactions.

REACTOR LO.

Reactor Lo. was a student in the Graduate School with some previous training in reaction experiments. Both in the character of his attention and time of reaction he was the steadiest reactor thus far considered. There was, too, little unsteady movement of any sort.

In series I the ordinary instructions were given to lift the finger on hearing the stimulus. In the remaining series, II-VI, the reactor was instructed to fix his attention on the coming stimulus and to let the reaction movement take care of itself. Lo. was selected for this type of reaction since he stated that in his earlier tests he had found that type the most natural one. After the first series he stated that his attention had been entirely on the stimulus. This series, then, is included in the general sensory averages (Table VIII). There is a gradual decrease in the reaction time through the series,—from 159σ in the first series, to 121σ in the last. The average reaction time for the six series is 141σ. This is well above the lower limit for sensory times taken with the spring key. To show more definitely the change in reaction times through the series, these were distributed according to their frequency in the first and in the second halves respectively. Curves showing these results, together with a summary curve of the whole series, are given in Fig. 14. Curve A represents the distribution for series I-III and shows a very good concentration of the times about 140σ and 160σ respectively. There are, however, a few scattering times from 90σ to 240σ. Curve B, giving the frequency in series IV-VI, yields a remarkable concentration of the times in the region between 120σ and 140σ with almost no scattering. As we would expect from such a curve, the mean variations are low. The average mean variation for the whole series is only 14.8. In series IV it is only 8.

Such curves, together with the summary curve C, when taken with the average reaction time and the introspections indicate clearly a sensory series of a very regular character. Lo. simply gained greater speed by practice with this particular kind of apparatus, to which he was at first not accustomed. With this increase in speed went also an increase in regularity. The intro-

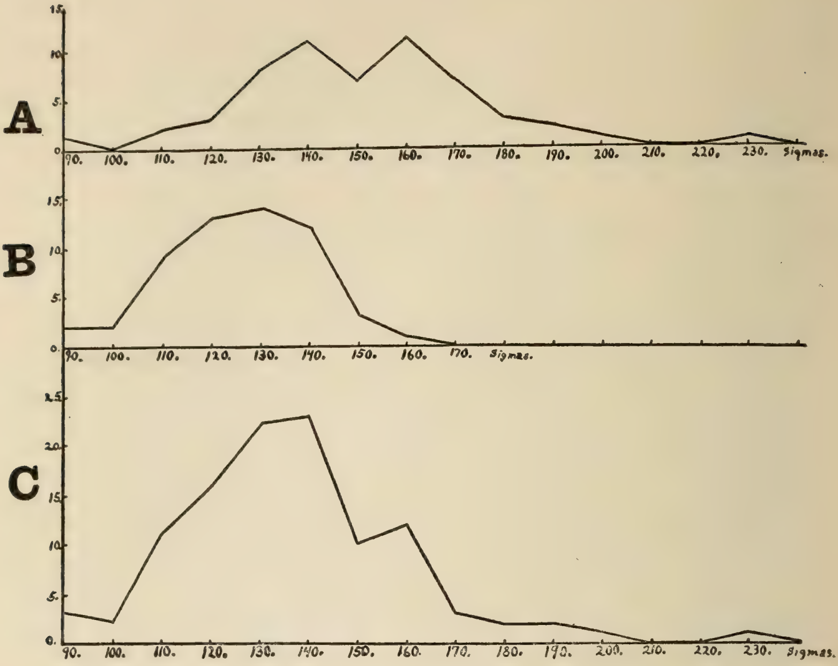


Fig. 14.

Reactor Lo.—Frequency curves of reaction times. Curve A. shows the frequency curve for the first half of the series. Curve B. shows the frequency curve for the second half of the series. Curve C. shows the frequency curve for the entire series.

TABLE VIII.—*Average times and mean variations. Reactor Lo.*

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	1.8	172.5	24.2	159.4	25.8	143.5	26.3
II	2.3	172	18.3	152.9	16.6	140.7	26.4
III	2.3	160.6	15.1	151	13.9	130.7	14.4
IV	2.0	144	9.6	132.5	8	116.1	12.2
V	2.1	140.9	11.1	128.8	11.2	120	10.9
VI	1.9	132.7	15.2	121.9	13.7	113.7	9.8
Averages	2.1	156.6	15.5	141.	14.8	127.4	16.6

spections show a remarkable lack of fluctuation in the attention. Out of ninety-six introspections only fourteen state a departure from the normal sensory type. Seven of these cases show that the attention wavered between the stimulus and the movement. Seven state that the attention was entirely on the movement. Three of these cases of motor attention seemed to have caused a slight distraction, for Lo. states that the time seemed long. One could hardly expect a more regular series of the sensory type.

Qualitative Results.—The pressure curves of Lo.'s reactions are likewise the most regular and steady of any discussed so far (Fig. 13). Very few marked changes in direction of the curves appear at the critical points. The changes at the warning signal are for the most part of one kind, namely, a slight increase in the amount of pressure, which is maintained as a level line to the beginning of the final reaction. The final reaction movements are largely of the antagonistic type. The decrease in reaction time is accompanied by a slightly sharper type of slight antagonistic reaction. Curves U* and V* show this distinction, so far as the form of the movement is concerned; the times are in these cases reversed by other causes. No reactions of type U* appear after series I, all but four of the antagonistic reactions being of type V;* three of type Z.*

A detailed summary of the forms of movement in Lo.'s series appears in Table IX. Series I-III and IV-VI were summarized separately. There is almost complete identity between the forms of movement in these two series. A description taken from the total summary will, therefore, be sufficient. Over one-half of the records are level before the warning, the rest being divided between a rise, 19/117, and a fall, 30/117. In nearly two-thirds of the cases the pressure is changed at the warning signal. The pressure is level after the warning signal in two-thirds of the records. The remaining one-third is divided evenly between a slight rise and a slight fall. In over one-half of the records the pressure is level towards the end, but there are more cases of increase in the pressure, 30/117, than of decrease, 13/117; there are a few cases of waviness, 11/117. In no case did a reaction before the stimulus occur. This we would expect in a sensory series of this uniform type. In nearly two-thirds of the records the final reaction movements were of the slight antagonistic type (V,* Fig. 13). About one-third were sharp upward movements

TABLE IX.—*Summary of the forms of movement. Reactor Lo.*

Key · *Series No.*	*I*	*II*	*III*	*Total I-III*	*IV*	*V*	*VI*	—	*Total IV-VI*	*Total I-VI*
Rise before warning	3	2	4	9	3	5	2		10	19
Fall before warning	2	5	5	12	7	6	5		18	30
Level before warning	12	13	8	33	9	8	12		29	62
Wavy before warning	3		2	5	1				1	6
Change at warning	13	9	11	33	14	14	9		37	70
No change at warning	7	11	8	26	6	5	10		21	47
Reacted to warning				0					0	0
Marked rise after warning				0					0	0
Medium rise after warning				0 } 8					0 } 7	0 } 15
Slight rise after warning	3		5	8	3	4			7	15
Marked fall after warning				0					0	0
Medium fall after warning	1			1 } 9		2	2		4 } 7	5 } 16
Slight fall after warning	3	2	3	8	1	2			3	11
Level after warning	12	17	9	38	15	9	17		41	79
Wavy after warning	2	1	2	5	1	2			3	8
Marked rise towards end				0					0	0
Medium rise towards end				0 } 5					0 } 8	0 } 13
Slight rise towards end	3	1	1	5	4	3	1		8	13
Marked fall towards end				0		2			2	2
Medium fall towards end		2	2	4 } 15	2	2	3		7 } 15	11 } 30
Slight fall towards end	4	5	2	11	3	1	2		6	17
Level towards end	10	12	14	36	8	8	11		27	63
Wavy towards end	3			3	3	3	2		8	11
Sharp reaction				0					0	0
Sharp reaction	9	10	3	22	3	9	9		21	43
Gradual reaction	1			1					0	1
Slight antagonistic reaction	10	10	14	34	16	8	9		33	67
Marked antagonistic reaction			2	2 } 36	1				1 } 35	3 } 71
Gradual antagonistic reaction				0		1			1	1
Double reaction				0		1	1		2	2
Number of reactions in series	20	20	19	59	20	10	10		58	117

(type Y*). Of the rest, three were of the marked antagonistic type Z,* one of the gradual antagonistic and two were double reactions. One of these is shown in type W.*

We may now consider, first, under what conditions of attention the more unusual forms of movement occur, and second, what forms of movement accompany the unusual types of attention, provided all of these are not included in the first distribution. There are eleven records which show waves towards the end. These indicate a tightening of the muscles in anticipation of the reaction movement. Such waves, we have found with the other reactors, appear more naturally with the motor type of attention. Of these, one was caused by a distraction, Lo. stating that he was watching the movement of his finger on the key. Five others occurred at times when the attention was divided between stimulus and finger. The remaining five occurred at times of ordinary sensory atention. Of the two double reactions, one occurred as we have seen, type W*, in a moment of distraction and the other in one of divided attention. Of the three marked antagonistic reactions, one occurred when the reactor's attention was on his finger and one during distraction (seeing the operator's movements). In the third marked antagonistic reaction the reactor stated, "Sensory; slow." This reaction was not slower than the average, but the antagonistic phase was longer. The only gradual antagonistic reaction in the entire series occurred when the reactor was asked to think about his breathing. It is interesting to notice that, though the attention was almost entirely withdrawn from the experiment and the reaction time increased to 350σ, yet the form of the final movement still remained the same, simply being more gradual in type.

Considering, now, the eighteen unusual introspections appearing in Lo.'s series, we find that only six are accompanied by the more usual forms of reaction movement; leaving a good two-thirds of the cases, in which unusual introspections involved unusual forms of movement.

Summary.—1. Lo. was a reactor of the sensory type.

2. The form of the final reaction movement was largely of the antagonistic type with the level or falling line preceding it.

3. Any marked change in the character of the attention was accompanied in most cases by a marked change in the type of the reaction movement.

REACTOR W.

Reactor W. was myself. I had had considerable training in reaction experiments and had used the spring key in a number of series taken in a previous year. I was almost a practiced subject, though I had never practiced for a long time with one type of attention. In order to obtain a series from myself, it was necessary to use the other reactors as operators. They knew nothing about the technique of the experiment, and the first five or six series were therefore taken while they were being instructed in the use of the apparatus. At no time during these series did any of the men become so proficient in manipulation that I could feel entirely free from responsibility. They were constantly meeting with difficulties in which they needed assistance, either verbal or manipulatory. This could not help being a distraction. It was only with an effort that I could keep my attention on my own work of reacting.

In the reactions taken the preceding year, I had found it easier to maintain the motor rather than the sensory type of attention. For this reason I began directing my attention on the movement to be made. Series I-VI, however, are of little value as motor series, since my attention was so much taken up with assisting in the operations. Series VII is the beginning of a more normal motor series. As late as series X I stated, however, "Somewhat distracted by having to assist the operator with the apparatus from time to time." This is the last series in which such a statement occurs.

The general character of my attention was as follows: I paid very little attention to the warning signal. When the motor was started my attention turned at once to a muscular preparation of my hand and especially my forearm for the reaction movement. This type of attention and preparation became steadily more pronounced up to a certain point and then changed. The point of greatest preparation we may call the climax of attention and was reached about two seconds after the warning, or more often two seconds after the starting of the motor. The slight variations in the time to the climax resulted from varying degrees of concentration of the attention. If the stimulus did not come at or before the climax, the attention would turn toward the stimulus. If it then came before the existing muscular preparation had had time fully to change, the reaction, though doubtless predominantly sensory, would be much faster than a normal sensory reaction without the muscular preparation. If the stimulus was still longer delayed, until the muscular preparation had quite altered, I was not ready and the reaction was slow. The final movement was in nearly every case a quick upward jerk of the forearm from the elbow, with almost no shoulder movement.

Quantitative Results,—Motor Series.—Twenty-three series of the motor type were taken, covering six weeks and averaging four series a week. The total number of reactions was 409. A summary of the reaction times is given in Table X. There is no marked effect of practice through the series. The reaction times at the end of the series are shorter than those at the beginning but there is a decided lack of uniformity in this decrease. The development seems to go forward in waves, each succeeding trough being lower than the one before. The longest steady decrease in time is from series XVIII-XXIII. In these last series the conditions were the most favorable. The average reaction time for the twenty-three series was 120σ. Omitting the first six series from the total average brings it down to 112σ. This time indicates a motor type of reaction.

The reaction times were distributed according to their frequency in the whole series and in the first and second halves of the whole series respectively (Fig. 15). Curve A shows the distribution for series I-XI. The most frequent times for these series lie in the region of 120σ and 130σ. There are a number of scattering times from 140-180σ. A large percentage of the reactions indicate, however, the motor type. The distractions already spoken of in the earlier series were responsible for their scattering times. Curve B gives the distribution for series XII-XXIII. The most frequent times in these series lie in the region

TABLE X.—*Average times and mean variations. Reactor W.*

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	1.5	152.	21.6	134.3	22.8	127.5	17.6
II	1.2	180.8	48.6	162.9	48.7	148.2	46.3
III	1.4	154.7	16.2	144.2	17.2	130.6	15.6
IV	1.4	138.3	11.7	120.4	15.4	105.2	22.4
V		142.6	17.3	130.3	16.2	116.3	18.2
VI	2.0	174.7	36.7	158.4	34.9	145.	34.1
VII	1.9	151.6	18.1	143.1	23.	131.9	18.1
VIII	2.3	137.6	21.9	120.2	23.8	111.4	26.8
IX	2.4	137.5	16.0	123.	23.3	109.6	20.8
X	1.8	138.5	16.3	115.2	18.4	111.3	20.6
XI	2.4	126.2	14.	111.4	20.8	105.7	22.
XII	2.4	128.9	8.8	115.2	10.2	115.2	10.2
XIII	2.5	143.5	17.1	123.1	14.9	122.2	13.7
XIV	1.6	140.6	16.	121.4	18.7	116.	15.5
XV	1.4	128.3	19.6	102.5	24.1	102.5	24.1
XVI	2.2	110.	13.7	84.6	18.9	77.5	22.1
XVII	2.8	134.	19.4	114.9	16.1	98.1	29.6
XVIII	1.5	138.	25.5	122.1	21.4	110.4	20.9
XIX	2.2	138.3	16.11	120.5	16.1	110.5	21.6
XX	2.0	131.3	10.6	115.8	12.	109.2	15.2
XXI	1.7	118.8	12.	100.	11.2	89.5	21.
XXII	1.7	118.2	6.88	94.	13.5	69.7	38.3
XXIII	2.0	108.8	9.9	87.2	11.5	77.7	17.4
Averages	1.8	137.9	18.02	120.2	19.7	110.4	22.2

of 100σ. There are very few times above 130σ. This curve is very sharp in form and shows a good concentration in the region indicating a motor type of reaction. It also shows a reduction in the average time between the first and second halves of the whole

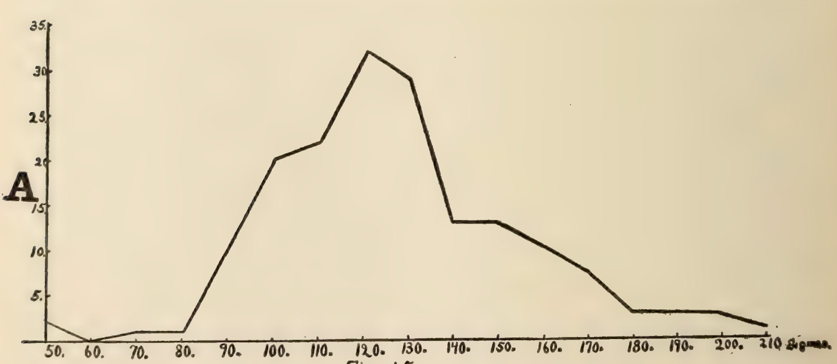
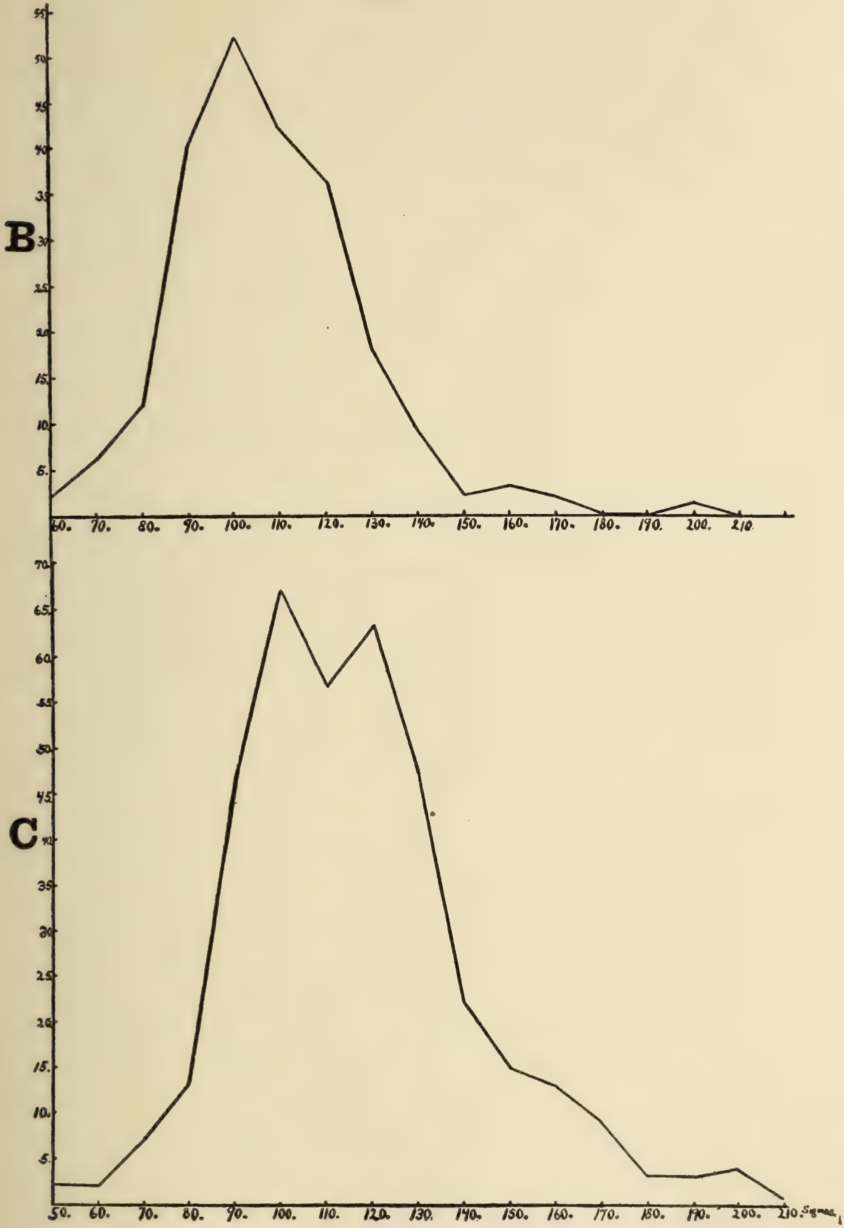


Fig. 15.

W

Fig. 15.



Reactor W.—Frequency curves of the reaction times. Curve A. shows the frequency curve for series I-XI. Curve B. shows the frequency curve for series XII-XXIII. Curve C. shows the frequency curve for series I-XXIII.

series. Curve C combines the two previous curves. The times are very well concentrated in the region between 90σ and 130σ. These curves tally with my introspections that except in the cases of distraction from without, the attention was of the motor type. The increase in the average reaction times in series II, VI, XIII, XVII and XVIII was due to the appearance of one or two long reaction times which occurred at times of partial distraction, rather than to a general altered state of attention in these series.

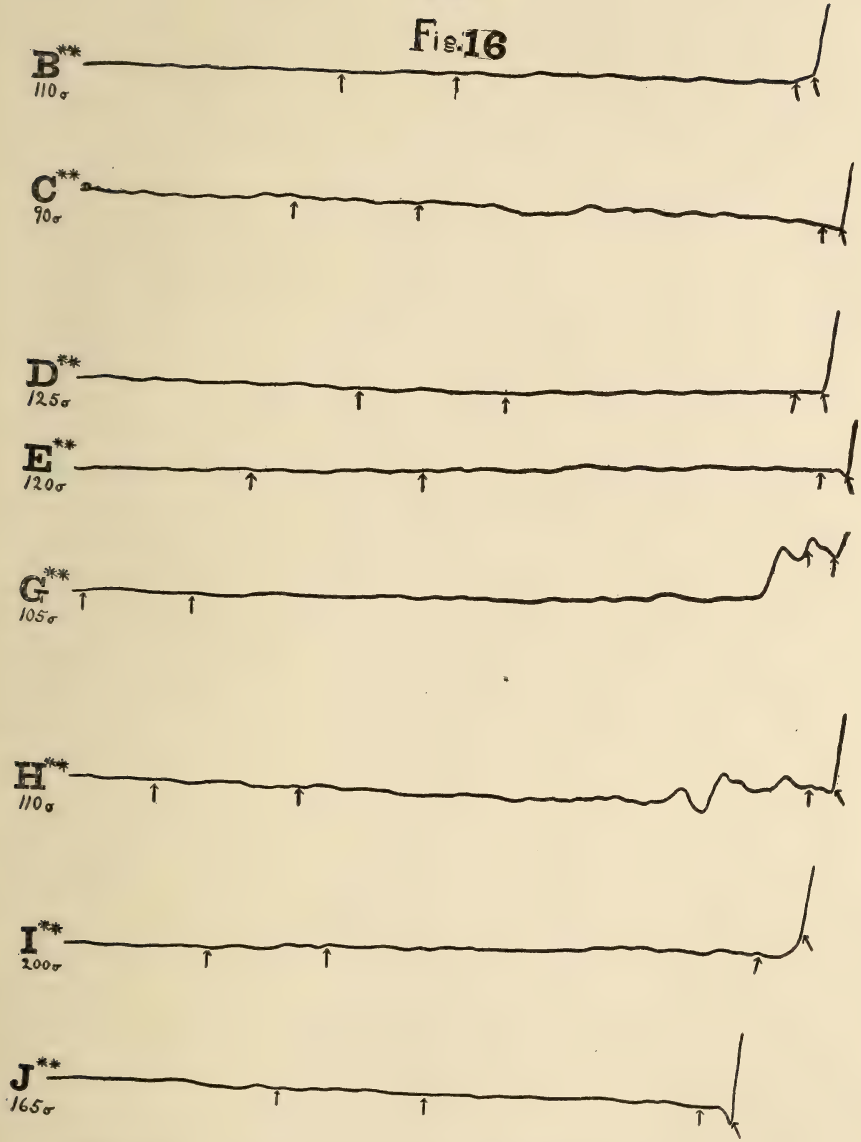
The average mean variation for the entire series is 19.7. This is somewhat larger than that of the other reactors after practice. Leaving out the first six series, which contain some of the largest mean variations, the average mean variation is 17.5. The mean variation reaches its lowest point, 10.2, in series XII. The largest single mean variation is in series II (48.7).

Qualitative Results.—Only twenty-two series are reported in the study of the forms of movement. In series XII no pressure curve was recorded, owing to an error on the part of the operator. A summary of the forms of movement in the whole twenty-two series and in the first six series, separately, is given in Table XI.

A general survey of the entire series shows that the records are usually very steady, especially in their earlier parts; that there is little change at the warning signal and that the final movement is mostly of the sharp type. Three-fourths of the records are level before the warning signal, showing a steady, even pressure. Nearly all of the remaining records, 87/409, increase the pressure in this period. In over half of the cases there is no change at the warning signal, 228/409. Where a change occurs it is usually from an increased or even pressure to a decreased pressure. In half the cases the line is level after the warning signal; in one-fourth the pressure is decreased and in one-fifth of the cases it is increased. There are only a very few cases of waviness in this period, 25/409. Towards the end the number of cases of level, steady pressure, has decreased to 166/409. The number of cases of a rise has increased, 113/409, as has also that of a fall, 97/409, and that of waviness, 43/409. Nearly two-thirds of the final reaction movements are of the sharp types shown in types B,** C,** and D,** Fig. 16. Only one-sixteenth of the total is of the

W

Fig. 16



Reactor W.—Pantograph reductions of the pressure curves.

gradual type, I.** Nearly one-fourth are antagonistic, most of them, however, of the slight type, E.** There are only seven cases of marked antagonistic reaction type, J,** and only seven of the gradual antagonistic type. There are nine double reactions of types G** and H.**

Since the first six series differed so much from the remaining ones, they were summarized independently in order to see to what extent the forms of movement found in them changed the character of the total summary (see Table XI). Their chief influence is seen in relation to the final reaction movement. Only one-sixth of the total number of sharp reactions and one-eighth of the total number of gradual reactions appear in these series (which contain, however, more than one-fourth of the reactions of the entire twenty-two series qualitatively analyzed). It is in the number of antagonistic reactions that the greatest difference, however, occurs. More than one-half, 57/97, of the antagonistic reactions appear in these six series. This leaves only thirty-five slight antagonistic reactions, two marked and three gradual antagonistic reactions outside of the first six series,—a total of forty in 301 reactions. This bears out the conclusion already reached in the cases of the earlier reactors, that fewer antagonistic and more sharp reactions occur as the reactions become more motor in type. The curve plotted in Fig. 17 shows how the

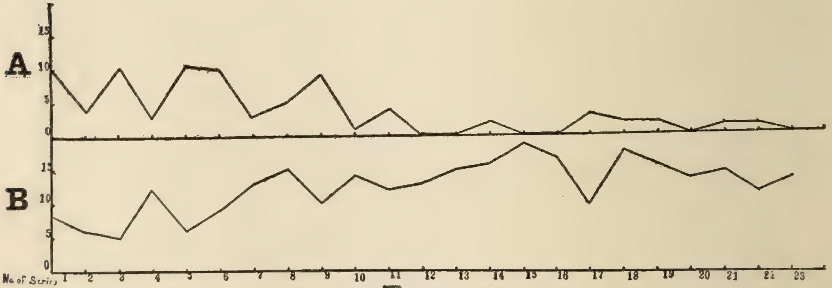


Fig. 17

Reactor W.—Frequency curves showing the relation between the number of antagonistic reactions and sharp reactions through the series. Curve A. shows the frequency of the antagonistic reactions; Curve B. shows the frequency of the sharp reactions. The number of antagonistic reactions is decreased and the number of sharp reactions is increased through the series.

Key / Series No.	I	II	III	IV	V	VI	Total I-VI		VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	XX	XXI	XXII	XXIII	Total I-XXIII	
Rise before warning	2	0	0	0	1	0	3		0	0	3	0	0	0	0	0	0	1	0	0	0	0	0	1	2	10	
Fall before warning	3	9	18	0	0	0	30		2	1	0	5	3	0	2	0	5	5	1	1	7	12	6	6	1	87	
Level before warning	12	2	3	15	10	18	69		17	19	16	9	15	0	14	20	15	13	16	18	12	6	14	13	15	301	
Wavy before warning	1	3	0	0	0	2	6		0	0	1	0	0	0	1	0	0	0	0	1	0	0	0	0	1	10	
Change at warning	11	11	12	4	10	6	54		5	9	10	4	7	0	8	6	7	7	8	8	6	6	12	12	11	180	
No change at warning	7	3	9	11	10	14	54		14	11	10	10	11	0	9	14	13	12	9	12	13	12	8	8	8	228	
Reacted to warning	0	0	0	0	0	0	0		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
Marked rise after warning	0	0	1	0	0	0	1		0	0	0	0	0	0	0	0	0	0	1	0	0	1	0	0	0	3	
Medium rise after warning	1	1	5	0	3	0	10	39	0	0	5	0	0	0	2	0	0	0	4	0	0	0	0	0	2	23	98
Slight rise after warning	5	0	12	3	6	2	28		0	1	10	1	7	0	1	6	5	4	2	5	0	1	0	0	1	72	
Marked fall after warning	0	0	0	0	0	0	0		0	0	0	0	0	0	0	0	0	0	0	0	0	1	1	0	1	3	
Medium fall after warning	3	1	0	0	0	1	5	16	0	0	0	2	0	0	0	0	0	0	0	0	4	7	4	5	3	30	79
Slight fall after warning	4	5	0	0	1	1	11		1	0	0	3	1	0	3	1	1	2	1	1	4	4	6	6	1	46	
Level after warning	3	7	2	12	10	13	47		16	18	4	9	9	0	11	13	14	9	5	13	10	4	9	8	9	208	
Wavy after warning	2	0	1	0	0	3	6		2	1	1	0	1	0	0	0	0	4	4	1	1	1	0	1	2	25	
Marked rise towards end	0	0	3	1	1	1	6		2	1	0	0	0	0	0	0	1	1	7	0	1	1	0	2	0	22	
Medium rise towards end	3	4	6	3	0	1	17	43	3	1	2	1	2	0	2	4	4	3	3	1	2	1	1	2	0	49	113
Slight rise towards end	5	2	6	2	2	3	20		2	1	0	3	2	0	2	1	3	4	1	1	0	1	0	0	1	42	
Marked fall towards end	0	0	0	0	0	0	0		1	0	0	0	0	0	0	0	0	0	0	0	0	0	1	2	1	5	
Medium fall towards end	3	1	1	1	1	2	9	17	0	1	2	1	4	0	1	2	0	2	0	2	4	5	3	3	6	45	97
Slight fall towards end	1	1	2	1	2	1	8		0	0	3	2	3	0	2	0	1	3	0	2	4	7	6	4	2	47	
Level towards end	5	6	3	7	10	10	41		11	11	13	8	4	0	7	12	10	5	5	13	6	3	7	5	5	166	
Wavy towards end	1	0	0	0	4	2	7		0	5	0	0	3	0	3	1	1	1	9	1	3	0	2	3	4	43	
Reacted before stimulus	0	0	0	0	0	0	0		0	0	0	0	2	0	1	0	0	0	1	0	0	1	0	0	0	5	
Sharp reaction	8	6	5	12	6	9	46		13	15	10	14	12	0	15	16	19	17	10	18	16	14	15	12	14	276	
Gradual reaction	0	1	2	0	0	0	3		2	0	0	0	2	0	2	2	1	1	1	0	1	1	1	5	2	24	
Slight antagonistic reaction	10	4	10	3	11	10	48		3	5	9	1	4	0	0	2	0	0	3	2	2	0	2	2	0	83	
Marked antagonistic reaction	0	2	1	0	2	0	5	57	0	0	0	0	0	0	0	0	0	0	1	0	0	1	0	0	0	7	97
Gradual antagonistic reaction	0	1	3	0	0	0	4		1	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1	7	
Double reaction	0	0	0	0	1	1	2		0	0	0	0	0	0	0	0	0	1	2	0	0	1	2	1	0	9	
No. of reactions in series	18	14	21	15	20	20	108		19	20	20	15	18	0	17	20	20	19	17	20	19	18	20	20	19	409	

TABLE XI.—*Summary of forms of movement. Reactor W.*

number of antagonistic reactions decreases and the number of sharp reactions increases through the entire series.

The forms of the final reaction movement were also distributed according to the character of the attention. The sharp reactions occurred at times of normal, motor attention, except in eighteen cases. Thirteen of these exceptions came at times of lowered attention and five at times of sensory attention. A careful distribution of the slight antagonistic forms of movement shows that of the thirty-five cases which occurred in series VII-XXIII, twenty-three came at times of special effort and fast reaction times; nine occurred at times of lowered attention and three at times of very irregular pressure towards the end. The two marked antagonistic reactions occurred when there was an attempt to check a reaction before the stimulus. All of the cases of double reactions occurred with introspections of "reacted too soon." The nineteen gradual reactions were due to two causes. In nine of the cases there was an anticipation of the stimulus which resulted in an extreme preliminary rise, giving the final movement a gradual form. This type we have already noticed in the motor series of reactor M. In the remaining ten cases the attention was distracted and the movement was very slow, the time likewise being slow.

The types B,** C,** and D,** in Fig. 16, show three different forms of the sharp reaction movement. Type D** occurs with medium motor attention and medium reaction time. In such a reaction there is no extreme muscular preparation. In type C** the extreme type of motor attention is present, together with an extreme degree of muscular preparation, which takes the form of an increased innervation of the extensor muscles resulting in an increased downward pressure on the key. In type B** there is the anticipation of the stimulus accompanied by a muscular preparation of the opposite type,—a gradual inhibition of the extensor muscles resulting in a decrease in pressure on the key.

At the close of the motor series and after an interval of nearly three months, three series were taken with the attention on the stimulus. The average time for the sixty reactions was 177σ, with a mean variation of 29. The frequency curve of the reaction times is given in Fig. 18, and shows a wide distribution of times between 130σ and 200σ, with scattering times from 110σ to 270σ. This curve is very characteristic of a sensory type of reaction in its early stages. A detailed comparison of the sensory with the motor times is of little significance because of the small number of sensory reactions. I found it very difficult to keep my attention consistently on the stimulus during these reactions, since all my training had been in the other direction. In the motor series it turned directly, when the electric motor was started, to a muscular preparation for a rapid movement. In the sensory series the attention went first to the warning signal; after the warning, to the coming stimulus; and often, after the stimulus; to the reacting finger.

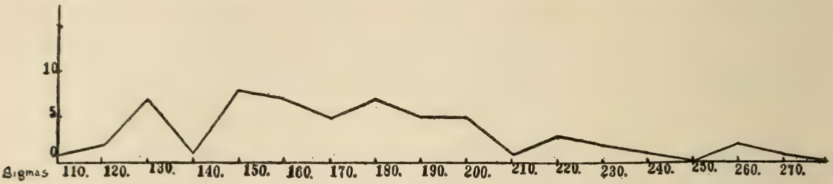


Fig. 18

Reactor W.—Frequency curve of the reaction times in the sensory series.

Qualitative Results—Sensory Series.—The summary of the forms of movement occurring in my three sensory series is given in Table XII. This summary shows the same characteristics found in the sensory series of the other reactors and differs from my own motor series in many marked respects. In the period before the warning, in nearly two-thirds of the reactions, there is an increase in pressure, while only one-sixth is level. In marked contrast to the motor series, all but six of the records show a change in pressure at the warning signal. In the motor series more than one-half of the records were level after the warning, and of the remaining there were many more examples of decreased than of increased pressure. In this sensory series more than one-half of the records have an increased pressure after the warning, while one-quarter are level and only one-tenth have a decreased pressure. There are almost no examples of waviness in this period. Towards the end of the record nearly one half show an increased pressure (as against less than one-fourth in the earlier series), one-quarter have a decreased pressure and one-sixth are level. No reactions before the stimulus occur. The number of final reaction movements is divided nearly equally between the sharp type of reaction, 28/60, and the antagonistic type of reaction, 24/60, as against a ratio of three to one in the motor series. Of these antagonistic reactions 16/60 are of the slight and 8/60 of the gradual type. There are no marked antagonistic reactions and no double reactions. A comparison of the forms of movement with the introspections shows that an increased pressure after the warning and towards the end, with a slight antagonistic reaction movement, accompanies a normal sensory attention and a normal sensory reaction time. Reactions

TABLE XII.—*Summary of forms of movement. Sensory series. Reactor W.*

Key *Series No.*	*I*	*II*	*III*	*Total*	
Rise before warning	2	3	2	7	
Fall before warning	9	14	13	36	
Level before warning	5	3	2	10	
Wavy before warning	4	0	3	7	
Change at warning	18	18	18	54	
No change at warning	2	2	2	6	
Reacted to warning	0	0	0	0	
Marked rise after warning	0	0	0	0	
Medium rise after warning	2	1	1	4	8
Slight rise after warning	2	1	1	4	
Marked fall after warning	0	1	1	2	
Medium fall after warning	3	7	2	12	35
Slight fall after warning	9	4	8	21	
Level after warning	3	6	6	15	
Wavy after warning	2	0	1	3	
Marked rise towards end	1	2	2	5	
Medium rise towards end	2	4	2	8	14
Slight rise towards end	0	1	0	1	
Marked fall towards end	2	1	1	4	
Medium fall towards end	4	4	4	12	27
Slight fall towards end	2	3	6	11	
Level towards end	3	5	3	11	
Wavy towards end	6	1	2	9	
Reacted before stimulus	0	0	0	0	
Sharp reaction	10	10	8	28	
Gradual reaction	2	3	3	8	
Slight antagonistic reaction	4	6	6	16	
Marked antagonistic reaction	0	0	0	0	24
Gradual antagonistic reaction	4	1	3	8	
Double reaction	0	0	0	0	
Reactions in series	20	20	20	60	

of the delayed sensory type having times over 200σ are accompanied almost always by the gradual type of antagonistic reaction. Of the twenty-eight sharp reactions, thirteen occur at times of motor attention, five at times of delayed sensory attention, and ten at times of apparently normal sensory attention.

The difference in the effect which the warning signal had in the different series is easily explained by my difference in attitude towards the warning. In the motor series I paid little attention to the warning signal, but began my muscular preparation for the reaction with the starting of the electric motor. In the sensory series I always waited for the warning signal before turning my attention to the stimulus. This change in the direction of attention was accompanied in nearly every case by a change in the character of the pressure.

Summary.—A study of my series has shown in general the same results which we found in our study of the other reactors, namely:

1. That no change in the form of movement due to practice can be discovered as the series progresses.
2. That definite changes in the form of movement accompany changes in the direction and the amount of attention.
3. That the forms of movement found more extensively in the sensory series occur also in the motor series with sensory attention or with lowered attention.
4. That an increase in pressure throughout the record and an antagonistic reaction is very characteristic of the sensory type of reaction, while a steady pressure or a decrease in pressure throughout the record and a sharp reaction is very characteristic of the motor type of reaction.

REACTOR J.

Reactor J. was a trained reactor who had little difficulty in concentrating his attention on either stimulus or movement. The attempt was therefore made to establish as clearly as possible a difference in attitude between the two types of reaction. Two series of reactions, one motor and one sensory, were taken each day, the order of sequence being varied from day to day. Ten series, comprising 167 reactions, were had with the attention on the movement and nine series with the attention on the stimulus. Pressure records were obtained for only eight of these latter series (see Table XIV), totalling 120 reactions.

The quantitative summaries for these two types of reaction are given in Table XIII, and frequency curves for the reaction times in Fig. 19. Curve A gives the frequency distribution of the times in the motor series. It shows a very good concentration in the region between 120σ and 140σ. There are few scattering times either long or short. Curve B represents the frequency in the sensory series. It shows a typical curve for a sensory series in its early stages. There is a wide scattering of the times between 110σ and 330σ. There were a few even longer times, not shown in the curve. The greatest frequency lies in the region from 150σ to 230σ. The curve seems to indicate that the sensory type of reaction was not a natural one for this reactor who, however, believed it to be the more natural.

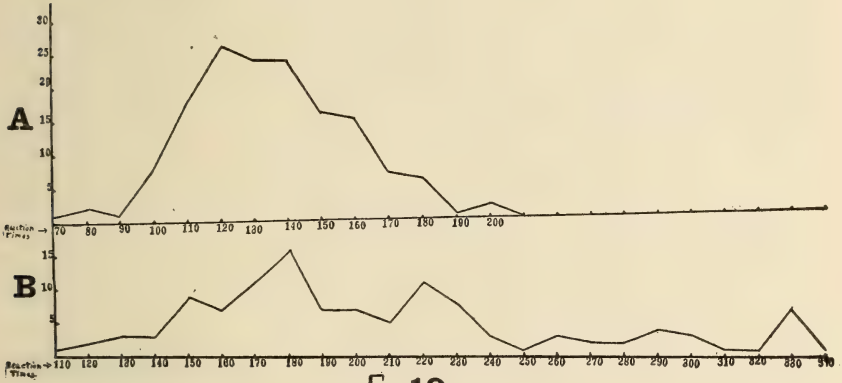


Fig. 19

Reactor J.—Frequency curves of the reaction times in the sensory and motor series. Curve A., motor. Curve B., sensory.

The average reaction time for the ten motor series was 135σ, with a mean variation of 20. This is a slightly longer time than that obtained in any of the other motor series. The average for the nine sensory series was 215σ, with a mean variation of 46. This is also a somewhat longer average time than that obtained from the other reactors. The large mean variation was due to the numerous delayed sensory reactions, caused by the great absorption of the reactor's attention in the stimulus.

In the first two of the sensory series the intensity of the stimulus was varied slightly from reaction to reaction, and the reactor was asked to state after reacting whether the stimulus appeared louder or softer than normally. This was done to aid him in keeping his attention on the stimulus. In the next three series the reactor was told not to pay so much attention to the change in the intensity of the stimulus. In the last four series the intensity of the stimulus was not changed. The changes in the intensity of the stimulus had little effect on the reaction time or the character of the movement.

TABLE XIII.—*Average times and mean variations*
Reactor J.

Motor Series

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning Time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	1.8	168	22.2	157.	24.4	140	11.7
II	1.6	152	13.8	141.8	15.2	133	9.5
III	1.5	144.2	21.1	127.	17.2	125.4	16.8
IV	1.5	160	21.1	151.4	20.1	138.8	22.7
V	1.3	157.1	18.4	139.3	25.2	126.7	26.6
VI	1.2	132.8	17.8	115.9	26.9	110.3	21.8
VII	1.2	144.4	12.8	122	26.6	120.2	29
VIII	1.4	146.2	12.2	134.4	13.3	122.5	17.5
IX	1.2	140	11.8	139.7	12.1	109.7	23.6
X	1.6	122	23.6	119.5	24.7	110.4	24.2
Averages	1.5	146.6	17.4	134.8	20.5	123.7	20.3

Sensory Series

		Chronoscope		*Corrected*		*Movement began*	
No. of Series	*Warning Time seconds*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*	*Time sigmas*	*Var.*
I	1.3	275.5	88.5	269.1	89.2	223.5	86.3
II	1.5	253.6	70	236.3	60.5	192.8	63.4
III	1.4	180	14	163.9	18.1	142.9	16.3
IV	1.4	246	44.2	237.5	45.9	183.4	39.5
V	1.3	217.5	48.7	202.4	50.5	175.8	38.3
VI	1.3	225.3	46.7	217.5	47.9	177.3	36.1
VII	1.4	222	36.4	216.5	33.8	161.9	16.5
VIII	1.3	204	41.6	199.5	40.4	177.4	40
IX	1.3	200	32	197	28.4	144.8	15.7
Averages	1.3	224.8	46.9	215.5	46	175.5	39.1

Qualitative Results—Motor Series.—A summary showing the forms of movement in the motor series is given in Table XIV. Records for fifteen of the reactions, reported in the qualitative summary, do not appear in the quantitative summary since they were reactions before the stimulus. There is a large number of cases of decrease in pressure, 74/167, as well of uniform pressure, 62/157, before the warning period, and almost no cases of increase in pressure, 9/167. There are a few cases of waviness, 22/167. There are no cases of increased pressure before the warning in the last four series. In almost every case the warning signal had a marked effect upon the amount of pressure,—a decrease lasting until the end of the record, 127/167 cases. There were but 12/167 cases in which the pressure was increased slightly after the warning signal, only a few cases of steady pressure, 22/167, and very few of waviness, 9/167. Towards the end of the record there is less uniformity. The cases of decrease in pressure are still the most frequent, 61/167, those of uniform pressure come next, 49/167, and those of an increased pressure next, 30/167. The cases of waviness have increased in this period, 28/167.

Nearly two-thirds of the final reactions are of the sharp type; one-third are antagonistic; while of the remainder, 12/167 are of the gradual type, and 4/167 of the double type. Of the antagonistic reactions 35/167 are slight, 17/167 marked, and 5/167 gradual. The introspections show that the gradual antagonistic reactions occur at times when the reactor checked a desire to react before the stimulus. The double reactions occurred when he endeavored to check a stronger desire to react before the stimulus. In two of the four cases of double reactions, the attempt was not successful. A second and even a third partial reaction occurred before the stimulus. Of the seventeen marked antagonistic reactions, fourteen occurred at times of lowered attention, one with the attention on the stimulus, and the other two when the introspections stated that the attention was good. Of the thirty-five cases of slight antagonistic reactions nineteen occurred at times of lowered attention and one with the attention on the stimulus; the remaining fifteen occurred with good motor

TABLE XIV.—*Summary of forms of movement. Sensory and motor series. Reactor J.*

Key *Series No.*	*Sensory*										*Motor*											
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*Total I-VIII*		*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*Total I-X*	
Rise before warning........	9	10	9	4	2	0	5	4	43		5	17	11	12	4	6	3	1	11	4	74	
Fall before warning	3	1	1	2	3	0	0	0	10		2	0	4	2	0	1	0	0	0	0	9	
Level before warning	7	10	10	5	11	10	5	0	58		11	2	4	6	11	7	3	9	5	4	62	
Wavy before warning	1	0	0	3	4	0	0	1	9		2	1	0	0	5	6	3	0	3	2	22	
Change at warning	18	16	15	13	16	7	8	5	98		16	18	19	20	20	19	8	10	19	10	159	
No change at warning......	2	5	5	1	4	3	2	0	22		4	2	0	0	0	1	1	0	0	0	8	
Reacted to warning	0	0	0	0	0	0	0	0	0		0	0	0	0	0	0	0	0	0	0	0	
Marked rise after warning..	0	0	0	0	0	0	3	2	5	44	0	4	7	7	3	8	4	2	13	7	55	127
Medium rise after warning..	5	0	0	1	1	2	2	2	13		0	8	11	9	9	7	0	4	2	3	53	
Slight rise after warning....	1	3	6	2	10	2	2	0	26		4	2	1	2	2	3	4	1	0	0	19	
Marked fall after warning...	3	0	0	0	0	0	0	0	3	30	0	0	0	0	0	0	0	0	0	0	0	12
Medium fall after warning	0	1	0	0	0	1	0	0	2		1	2	0	1	0	0	0	0	0	0	4	
Slight fall after warning.....	5	8	6	1	4	1	0	0	25		3	3	0	0	1	1	0	0	0	0	8	
Level after warning.........	6	9	8	6	4	3	3	0	39		12	1	0	1	4	1	0	3	0	0	22	
Wavy after warning	0	0	0	4	1	1	0	1	7		0	0	0	0	2	1	1	0	5	0	9	
Marked rise towards end....	0	0	0	0	0	0	1	1	2	22	0	0	0	2	0	3	1	0	3	2	11	61
Medium rise towards end....	5	0	0	0	1	0	1	1	8		0	3	4	5	6	4	0	1	3	2	28	
Slight rise towards end......	0	2	4	2	2	1	1	0	12		4	2	6	5	0	3	0	2	0	0	22	
Marked fall towards end.....	2	0	1	0	0	0	1	2	6	32	0	0	0	0	0	0	0	0	1	1	2	30
Medium fall towards end....	3	2	0	3	3	0	0	0	11		0	4	0	2	0	4	0	0	3	3	16	
Slight fall towards end......	3	4	2	1	2	3	0	0	15		1	3	3	1	0	1	1	2	0	0	12	
Level towards end..........	7	12	13	6	10	5	6	0	59		15	7	6	5	6	2	1	2	4	1	49	
Wavy towards end	0	1	0	2	2	1	0	1	7		0	3	0	0	7	2	6	3	6	1	28	
Reacted before stimulus	0	0	0	0	0	0	0	0	0		0	0	0	0	0	1	0	0	2	0	3	
Sharp reaction	0	6	3	0	5	0	4	0	18		10	14	18	14	9	14	5	2	1	6	93	
Gradual reaction	0	0	0	3	0	0	0	0	3		0	1	0	0	2	2	3	1	2	1	12	
Slight antagonistic reaction..	3	3	4	0	3	0	3	0	16	98	5	3	1	4	5	1	0	3	12	1	35	57
Marked antagonistic reaction	15	12	12	4	12	9	3	5	72		5	2	0	1	1	2	0	2	3	1	17	
Gradual antagonistic reaction	2	0	1	6	0	1	0	0	10		0	0	0	1	1	1	1	0	0	1	5	
Double reaction	0	0	0	1	0	0	0	0	1		0	0	0	0	1	0	0	2	1	0	4	
No. of reactions in series....	20	21	20	14	20	10	10	5	120		20	20	19	20	20	20	9	10	19	10	167	

attention and fast reaction times. One of the cases of distraction is interesting. The operator noticed that no antagonistic reactions had occurred in the first nineteen reactions of a given motor series. Just before reaction No. 20 he asked the reactor if he had noticed any antagonistic tendencies during the series. The reactor stated that he had not. In reaction No. 20 occurred the only antagonistic reaction in the series. The reactor did not, however, record any distraction.

Summary.—We find then, that in the majority of cases a definite form of reaction movement accompanies the motor type of reaction; that in many cases in which there is a departure from this type of movement there is also a change in the character of the attention; that the form of movement in the motor reaction is a steady decrease in pressure in the period before the stimulus and a simple sharp rise in response to the stimulus.

Qualitative Results—Sensory Series.—The summary sheet of the forms of movement in the sensory series (Table XIV) shows many points in marked contrast to those found in the motor series. Of the records 58/120 are level before the warning, while only 43/120 rise slightly. There are but few cases of an increase in pressure, 10/120, and of waviness in this period, 9/120. There are not as many cases of change in the character of the pressure at the warning signal as in the motor series, although 98/120 of the records show a slight change at this point, different in type, however, from that observed in the motor reactions. Of increased pressure after warning there are in the sensory series 30/120 as against only 12/167 cases in the motor series; of uniform pressure, 39/120 against 22/167; and of decreased pressure but 44/120 in contrast with 127/167 for the motor series. These decreases in the sensory series are, furthermore, very slight. It is apparent, then, that there is a much greater tendency towards greater pressure, in the period after warning, than there was in the motor series. Towards the end, 59/120 of the records are level, 32/120 show increased and only 22/120 decreased pressure. There is almost no waviness in this period, showing little anticipation of the stimulus, 7/120.

Of the final reaction movements 98/120 are antagonistic and of these a large majority, 72, are of the marked type; 16/120 are slight, and 10/120 are gradual antagonistic reactions. There are only 3/120 gradual reactions, all of which appear together in one series. There is only one double reaction, which occurs in the same series. After this reaction J. stated, "Motor attention." The introspections concerning the sharp reactions, 18/120, do not help us much in explaining the reason for their appearance. Twelve occur with introspections of "Sensory, fast"; five occur with introspections of "Sensory, slow"; one occurred at a time when the attention was on the movement.

Summary.—1. There is a fairly definite type of movement associated with the sensory type of reaction.

2. This movement consists typically in a uniform pressure or an increase in pressure in the period before the stimulus and an antagonistic reaction movement in response to the stimulus. When decreases in pressure occur the amount of decrease is very slight compared to that in the motor series.

Discrimination Reactions.—At the end of these series one series of the discrimination type was taken, in which the reactor was told to react to the sound of an electric hammer on his left and not to react to one on his right. The average reaction time of the nine reactions was 315σ, with a mean variation of 42. The same characteristics found in the sensory series appear in the discrimination series, but, particularly in the character of the final reaction movements, to a more marked degree. There are no sharp reactions. Seven of the reactions are of the marked antagonistic type, one is a slight antagonistic reaction, while one is a gradual reaction. In this discrimination series, then, in which the attention of the reactor was even more removed from the speed of the movement than in the simple sensory series, the reaction movement is more markedly of the antagonistic form.

IV. GENERAL DISCUSSION AND CONCLUSIONS

Smith (**5**) was interested in the form of final movement made by the reactor in response to the stimulus and in only one form of it—the antagonistic. Judd, McAllister and Steele (**3**) extended the study to an analysis and classification not only of the forms of final movement, but also of the types of movement made in the period of preparation and adjustment preceding the stimulus. Since this work was the first of the kind, the authors concerned themselves with a determination of *what forms of movement there were* rather than with a study of the conditions under which they occurred. They did not, however, as they themselves say (p. 168), get enough records from the different reactors to yield a satisfactory classification of the forms of movement according to individuals. In the present investigation, we have endeavored to classify the forms of movement according to the individuals and in relation to types of attention and to reaction times. In almost every case the reactors themselves knew nothing in detail of the object of the investigation.

In the general discussion of results which is to follow, while we believe that the conclusions reached are based on very clear evidence, both introspective and objective, and hold, therefore, for our particular reactors under the conditions imposed, we do not, of course, claim for them universal application. They do, however, throw considerable light on the problem of "reaction types" and show the sort of results which may be expected in this field of investigation. More than this they throw light on different types of reaction in the same individual.

The main conclusions which we have reached thus far may be summarized as follows:

1. In the practice series in which the reactors received no instructions except to remove the finger from the key on hearing the stimulus, the reaction times and the mean variations were decreased. The changes in the form of the reaction movement were found to correspond with definite changes in the amount and in the direction of the attention. There was also often a marked correspondence between the changes in form of movement and those in the reaction times; the times as well as the attention varied, that is, with the form.

2. In those series in which the reactors were trained to react in the sensory or motor manner, a marked difference was found between the forms of movement in the two types of series.

a. In the typical motor reaction, the pressure was decreased from the warning signal to the beginning of the final reaction to the stimulus; this final reaction movement was a simple, sharp rise.

b. In the typical sensory reaction the pressure was either uniform or increased from the warning signal to the beginning of the final reaction movement, which was then most often antagonistic in form.

3. The same forms of movement which appear as typical in the sensory reactions were found also in the motor series at times of lowered attention, of unintentional sensory attention or of distraction; for example, the final antagonistic reaction occurred, in motor series, when the attention deviated from a preparation for the final movement.

4. The same forms of movement which appear as typical in the motor reactions were found in the sensory series when the attention was turned unintentionally towards the movement.

5. The average correction in the reaction time which had to be made for the antagonistic form of movement was 30σ.

6. Very clear proof was found in the investigation that there were *types of reactors*. This difference in type involved not only the type of attention and the reaction time—the criteria of earlier investigators—but also the form of the movement.

In order to bring out these conclusions more clearly, we will take up a discussion of the different forms of movement occurring in the different periods of the reaction, showing how each of these forms of movement is related to the consciousness of the reactor and to the reaction times.

In the period before the warning, four types of movement were considered; a decrease in pressure, an increase in pressure, a steady pressure, and a variable pressure (giving a wavy line). Judd, McAllister and Steele found the cases of waviness the most frequent, of steady pressure next, with an increased and a decreased pressure following in the order named. The authors explained this on the ground that a wavy line represented a passive state in which no active tendency showed itself. In our records the wavy lines did not appear as often in this period as the level lines or the increases in pressure. This can be accounted for perhaps by the different conditions under which the experiments were performed. In the earlier work the drums carrying the smoked paper were rotated by hand. The subject would then not be aroused to any definite mental attitude until the giving of the warning signal. In our investigation the drums were turned by means of an electric motor and the beginning of a given record was marked by the starting of this motor. Although the motor caused little distraction, yet it placed the reactor in a more definite attitude of expectancy from the very first. This attitude was, in the case of one reactor, so noticeable that he asked why a second warning signal was used and stated that he paid no attention to the warning given by the telephone, since the starting of the motor acted as a warning. This the writer found to be true in his own case, the starting of the motor initiating a positive state of preparation which increased in intensity as the time for the stimulus approached. With most of the reactors, however, the period before the warning was one of waiting for the warning or of bodily and mental adjustment to the conditions of the experiment in general. In our discussion of the reactors in Part III, we laid little emphasis on the forms of movement in this period because the reaction period was usually not fairly begun until after the warning signal sounded.

At the warning signal the records were classified according to whether there was a change or no change in the pressure at this point. No attempt was made to classify in the tables the different forms of change. The important thing was to note that a change took place. As we have seen, in the case of every reactor except myself, some form of change occurred in the majority of records. In the discussion of the individual reactors we have stated what the nature of it was.

Summing up the individual statements, we find that the change in the pressure at the warning signal takes two forms. In the first place the signal seems to produce a slight "start" on the part of the reactor which shows itself in the record by a sudden change in the direction of the line and a quick return to the same or a different level. This change has the appearance of a small trough caused by a slight antagonistic movement or a low wave caused by a slight upward movement. Whether the movement is upward or downward seems to depend on the mental attitude and the muscular preparation of the reactor. If his attention and muscular attitude are already concerned with a preparation for the final reaction movement, the upward movement results. If his attention and muscles are simply concerned with holding down the key, the slight antagonistic movement results.

In the second place, the warning signal starts a definite and more permanent, though gradual, alteration in the degree of pressure, succeeding the brief trough or wave just mentioned. If the reaction be motor, this usually takes the form of a gradual decrease in pressure up to the arrival of the stimulus. If the reaction be sensory, the change becomes a gradual increase in pressure up to the arrival of the stimulus. These forms of pressure, introduced by the warning signal, receive more detailed discussions a little farther on.

In his work on Reaction Time and Attention, Bliss (**1**) quotes one of the reactors as saying: "Every warning is heard with a tendency to react." This statement was not borne out by the introspections of my reactors. It was only in exceptional cases that they were aware that the warning signal had produced any change in the pressure. Such cases occurred most often when the reactor's attention was wandering, the warning signal serving to bring back the attention suddenly to the problem before him. The records in such cases show marked partial reactions to the warning signal. Aside from introspective evidence, however, my results give an element of confirmation to Bliss's statement, since in nearly every case the warning signal *did* cause a partial reaction or a change in pressure. But the number of changes was as great for the sensory as for the motor type.

If the period before the warning may be called one of general adjustment, the periods after the warning may be called periods of expectancy. Here, as in the period before the warning, the records were divided into those showing increased pressure, decreased pressure, steady pressure and varying pressure (wavy line). A decrease in the pressure seems, as indicated above, to characterize a growing mental and muscular preparation on the part of the reactor for the final upward movement which he is soon to make. There were 760[1] such cases of decreased pressure in the period immediately following the warning. Over one-half of these were of the slight type, and 704 of them occurred in the motor reactions.

In the cases of increase in pressure the reactor's attention, which at the outset had been turned towards holding the finger on the key, is, at warning, shifted towards the expected stimulus. The muscular preparation of holding the key down is, however, continued, the sensations of the finger on the key tending reflexly to maintain and even increase the downward innervation. It does not appear to be a definite, voluntary preparation for a quick reaction, for the reactor's attention is directed away from the movement *towards the coming stimulus.* This is shown by the fact that most of the 296 cases of increased pressure occurred in the sensory reactions. Over half of them were of the slight type.

I am inclined to accept the conclusions of Judd, McAllister and Steele, that a level line indicates "a form of expressive activity." and not mere neutral passivity. It may occur in either the sensory or motor types of reaction, but the introspections show that it is less likely to appear when the attention is distracted or lowered. Its most frequent occurrence is in the motor reactions when the subject is not preparing to react as quickly as possible. A certain amount of muscular preparation takes place, but involves only sufficient innervation to maintain a constant position. I do not suppose that we can regard such an innervation as consisting of an equal innervation of both sets of antagonistic muscles, in view of the work of Sherrington (**4**), causing a rigid equilibrium of opposing forces, but rather as an innervation of one set of muscles, in this case the extensor muscles, uniformly maintained. The evidence shows that in the motor reactions of the level type, the attention is fixed then, either on the coming final movement, the accompanying muscular preparation not being sufficient to give rise to a preliminary upward movement; or on the finger, in which case the muscular activity is involved in an effort to hold the finger in place.

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[1] In the following summaries the results of two reactors are included which were omitted from the discussion because of lack of space.

In the sensory reactions, the attention of the reactor is on the coming stimulus. Since at the outset, however, as already described, the train of motor impulses required to hold the finger on the key had been started, this downward innervation would be reflexly kept up because of the constant pressure of the key against the finger. If, as with reactor H., this upward pressure of the key did not seem to furnish a sufficiently firm foundation, the pressure of the finger would be constantly increased. If the finger felt secure upon the key, a constant and uniform pressure would result.

The total number of level lines in the period after warning was 830. We do not mean, of course, absolutely level lines, as though drawn with a ruler. There is always more or less slight change in pressure and slight undulation. The lines do not, however, show any more than just noticeable irregularities.

The introspections show that a wavy line in this period sometimes occurred because the reactor was aware of a start at the warning signal and endeavored to regain his equilibrium. Sometimes it occurred when the attention was distracted or lowered. There are 305 cases of such wavy records.

The total period between the end of the warning signal and the beginning of the final reaction movement in response to the stimulus was, it will be remembered, divided into "the period after warning" and that "towards the end," the latter referring to that part of the curve just preceding the final reaction. We will now take up this period, "towards the end." In it a rise of the curve meant in almost every case a mental and muscular anticipation of the stimulus. It is in the motor reactions that this form of movement is most characteristic. The attention of the reactor had been on making a quick reaction. If the stimulus was delayed, or if the motor attention reached a climax too soon, a gradual upward movement was begun. This movement was not fast enough to break the circuit. Such a preliminary rise did not occur as often in the sensory reaction. Where it did occur it could frequently be explained by attention on the movement.

Where this was not possible, it was found in many cases to be due to such causes as irregularities in the pressure, or waves, rather than to a positive tendency to react. In all, there were 687 cases of a rise towards the end; 519 of these occurred in the motor series.

The cases of fall towards the end occurred most frequently in the sensory reactions. Here the reactor's attention was on the coming stimulus and the muscular activity was involved in holding the finger on the key. Many of the instances preceded a final antagonistic movement in response to the stimulus. Where cases of a fall occur with the motor reaction, they were found either at times of sensory attention or distraction, or else were due to marked irregularities in the pressure. There were 512 cases of a fall towards the end, of which 274 occurred in the sensory series.

There were 937 cases of a level line towards the end. Why should there have been a greater tendency for this line here than in the period after the warning? In studying the records, one fact became readily noticeable. In many records, regardless of whether the earlier parts had risen or fallen or had been wavy, there appeared this short, level piece of record. It seems possible by comparing the pressure here with the introspections to explain this on the supposition that with the imminent approach of the stimulus both the extensors and the antagonistic flexors were innervated to the mutual degree required for equilibrium. There did not seem to be introspectively any decrease of muscular innervation, but rather a firm setting of the muscles in preparation for the coming stimulus.

Nor can the wavy lines towards the end well be related to a passive conscious state. They are the muscular accompaniment of anticipation of the stimulus and are entirely different in form from the earlier waves. These were irregular in length and in amplitude. They were due largely to irregular muscular changes of hand, arm or body. The waves occurring towards the end, however, were very uniform in length (about 100σ) and in amplitude. That these waves were due to a more extreme degree of anticipation of the stimulus (accompanied by increased muscular innervation) than accompanied the level line is shown by the introspections and by the fact that nearly all occurred in the motor reactions where the pressure towards the end was being decreased. Such a distinction is brought out in the case of reactor J., in whose sensory reactions we find only seven cases of waviness at the end, while twenty-eight examples appear in the motor series.

What evidence can we furnish as to the type of reaction in which the premature reactions occur? Since only twenty-eight such reactions appear in the 2500 graphic records, they do not play a very important part in this investigation. Twenty-four of these twenty-eight occurred in motor reactions when the reactors showed specially strong muscular preparation and when their introspections recorded an extreme anticipation of the stimulus, usually because of a delayed stimulus. This seems to conform to the results of the majority of investigations in this field.

We turn now to a brief discussion of the final reaction movement. The sharp reaction represents the simplest possible type,—the quick lifting of the finger from the key in response to the stimulus. It occurs 1402 times, which is more than half the total number of reactions. As we have seen, this form of reaction appears most frequently in the motor series, 1144 times, leaving only 258 cases in the sensory series. Many of these latter have already been accounted for, by the introspections, as occurring when the attention had shifted to the movement. Our results do not by any means warrant the statement that a simple, sharp reaction will occur only when the attention is turned towards the movement. They do, however, mean that it is more *likely* to appear in the motor reaction. Further work with subjects more thoroughly trained may show that it will *always* occur in the motor reactions if no distractions appear.

Under this head of sharp reactions we have undoubtedly classified many different forms of movement. It represents, however, some such class as a biological species. Each of the individuals differs in some detail, but they all possess the majority of characteristics in common. These individual differences are related to the form of the movement just preceding and to the rapidity of the movement itself. A sharp reaction terminating a fall in the line presents an entirely different appearance from one terminating a rise or a level line. A sharp reaction made in 100σ differs from one made in 140σ.

On the physiological side, we may assume, since a great majority of the sharp reactions (1144/1402) occurred in reactions of the motor type, in which the preparation was for the final upward movement, that the retractor muscles were partially innervated and the corresponding extensors partially or wholly inhibited. The motor nerve paths to the retractors were, then, the more open when the stimulus came, so that the additional innervation went directly into these motor paths, causing the quick lifting of the finger from the key. In the cases of a rising line preceding the advent of the stimulus, characteristic of motor reactions, the upward movement had actually begun, giving an emphasized preparation for the final release into a sharp reaction.

There has been a great deal of discussion as to whether the motor reaction is a brain reflex or not. Our results may seem to point towards that view, and yet we must go one step further back and not leave out of account the period of expectancy and preparation preceding the final reaction in which the particular "set" for subsequent innervations and inhibitions is given. The first step of mental and muscular preparation would, in such event, be necessary to produce the second step of "reflex" action. This explanation accounts also for the fact that the simple, sharp reaction movement occurs more often in the motor reactions. It seems reasonable, too, to expect that wherever the motor type of attention exclusively prevails, the sharp reaction movement will occur. That it did not always appear at times when the reactor thought his attention wholly on the movement might be interpreted as due to faulty introspection, although, in the absence of fuller evidence on this point, I shall not insist on such an interpretation. And yet all who have taken the part of reactor in such a series of reaction experiments know how easy it is for the attention to shift to the stimulus at the last instant in a motor reaction or to the finger in a sensory reaction. When we add to this possibility of mixed attention the many chances for distraction, from either inner or outer causes, it seems almost strange that our results should be as uniform as they are.

No fine line can be drawn between what we have called the sharp reaction and the gradual reaction. It would be possible to find in the records of all the reactors a complete series showing gradual transitional forms from the most extreme form of sharp reaction to the most extreme type of gradual reaction. Only those reactions which are clearly of the gradual type have been included under this head. They may, as seen, be due to three different causes:

First, to a slow upward movement of the hand in response to the stimulus. This is the most usual type of the gradual reaction.

Second, to marked waves in the period just preceding the final movement, this movement constituting the ascending phase of one of the waves.

Third, to a rise in the line just preceding the stimulus, so rapid that it forms an obtuse angle with the final upward movement made in response to the stimulus. This latter form is rare and occurs almost entirely in the motor series of reactors M., W. and J.

The second form arises when the stimulus accidentally occurs at such a point in the already existing wavy line as to allow the final reaction movement to coincide with the upward phase of a regular wave. It is not, certainly, due to any lowering of the attention, since it often occurs when the reactor is actively anticipating the stimulus. The first form may be accounted for in two ways. It occurred most often in the first few series of each reactor when he was establishing the connections between the stimulus and the proper response. The times in these cases were long. Where it appeared in the later series it occurred at times of complete distraction or of very marked lowered attention. At such times the reactor raised his hand very gradually and the time was correspondingly lengthened. There were 204 cases of gradual reactions, 170 of these occurring under the conditions last described.

Forty-one cases of double reaction appear. Twenty-nine occur in the motor series, leaving only twelve for the sensory series. In these twenty-nine cases the introspections show anticipation of the stimulus. The impulse to react, involving a decrease in pressure, is strong enough to start a premature reaction which is, however, realized and checked until the stimulus arrives, when the second and full reaction takes place. The varying forms assumed by this type of reaction depend on the extent to which the first reaction was allowed to go before being checked, and also on the length of time between this preliminary reaction and the stimulus. In the case of complete premature reactions there is no tendency contrary to the impulse to react that is strong enough to evoke a realization that the reaction is under way until the complete reaction has occurred.

The antagonistic reactions have now to be considered. It may be well at the beginning to call attention to a slight difference in terminology between our work and the earlier work of Judd, McAllister and Steele (**3**). The authors there apply the term "gradual antagonistic movement" to the cases of increased pressure *after the warning signal*. They speak of the reactor's preparing for the final reaction by an antagonistic movement. In this work it seemed better to confine the term "antagonistic" to the final reaction movement itself, occurring after the giving of the stimulus. Since some of the antagonistic reactions were sharp and some were gradual, the term "gradual antagonistic reaction" has been applied to the latter. We have distinguished then, three types of antagonistic reaction: the slight antagonistic reaction, involving a sharp downward movement of slight extent; the marked antagonistic reaction, involving a sharp downward movement of marked extent; and the gradual antagonistic reaction, involving a gradual downward movement, and yielding a U-shaped depression in the curve in contrast to the V-shaped depression of the other two forms. Of course, no sharp line can be drawn between the slight and the marked antagonistic reaction. Yet in the majority of reactions it was easy to distinguish between them. The slight and the marked reactions do not shade into the gradual forms.

This classification was based on the general appearance of the reaction curve, but a more careful study of these reactions in relation to their introspections shows that they could be divided into four classes according to the conditions under which they occur.

1. *Normal antagonistic reaction:* this is usually of the slight type, occurs most often in the sensory series, and constitutes the majority of the antagonistic reactions.

2. *"Start" antagonistic reaction:* this occurs at times of extreme effort. The introspections show that the reactor's attention had been wandering or lowered and the stimulus brought the attention back with a start. It, too, occurred most frequently in the sensory series, and never in the motor series except under distraction. These antagonistic reactions are most often of the marked type.

3. *Wavy antagonistic reaction:* this is a pseudo-antagonistic reaction, occurring at the end of a period of waves. Since the reaction comes in the trough of one of the waves, which is sometimes slightly exaggerated, it has the antagonistic appearance.

4. *Gradual antagonistic reaction:* this form is due almost always to a slight attempt to react before the stimulus, which is checked just as the stimulus arrives. Had the stimulus been delayed an instant longer, this form would have assumed that of a double reaction.

Of these four classes, the wavy antagonistic reaction should doubtless be excluded, as is indicated in point 3 above. The wave after the signal is simply carried downward to a greater extent, and it would be difficult to say at what point the final reaction movement really began. It seems that only the first, second and fourth forms mentioned above may really be unambiguously called antagonistic reactions.

The experiments of Smith (**5**, pp. 56-57) to determine the cause of the antagonistic movement were repeated (see, also pp. 87-88, pp. 100-101 of the present article). Since it was not possible to change the position of the spring key without considerable modification of the apparatus, these test experiments were taken with a large tabour. In all of them the reactor was required to attend, as far as possible, to the expected stimulus.

In the first set of experiments, the tambour was placed vertically and the reactor, standing and facing at right angles to its surface, was required to place the back of his finger against it. When the stimulus occurred, he reacted by a horizontal movement with his arm involving the extensor muscles. The antagonistic phase of the movement still remained. The reactor next stood still fronting the vertical tambour which was, however, lowered so that the reactor's arm might hang at his side. The back of the index finger was in contact with the tambour. The reaction consisted of a quick backward swing of the arm and hand, involving the extensors, which is perhaps the strongest normal movement of the arm, since it is the one used by the running or walking animal. The antagonistic movement still appeared.

In another set of experiments the tambour was *inverted.* With the back of his finger against the rubber, the preliminary pressure being upward, the reactor responded to the stimulus by a simple downward movement of the hand. In the case of reactor W. the strain of keeping the hand pressed against the surface of the tambour was so great that very marked muscular waves appeared. With this procedure also the antagonistic movement still occurred, the reactors making a preliminary increased upward movement before the downward reaction.

The experiment was then tried of allowing the reactor to rest his finger passively on the surface of the tambour when the latter faced upwards. the only pressure on the rubber being the weight of the hand and arm. The reactor was instructed to react by pressing downward. In this case no antagonistic movement occurred. Again, a variation was tried by instructing the reactor to hold his finger down upon the tambour with every muscle rigid. In one instance the reactor was instructed to react to the stimulus by lifting the finger, and, in another, by pressing down. The antagonistic movement once more appeared when the finger was lifted, but did not appear when the reactor responded by pressing downward.

Let us briefly summarize, then, what we have found in regard to the antagonistic form of movement:

First, in the regular investigation the antagonistic movement occurred most often in the sensory series.

Second, when the antagonistic movement occurs in the motor series the introspections often indicate sensory attention, lowered attention or distraction.

Third, the special experiments just described showed that in a sensory series *the antagonistic reaction takes place with any set of muscles and in any position so long as the reactor, while pressing in one direction, reacts in the opposite direction.*

Fourth, that the antagonistic movement did not appear when the reactor placed his finger on an upward facing tambour and reacted by pressing downward, showed very clearly that *this movement does not occur so long as the reactor presses in a given direction and reacts in the same direction.* Titchener (**6**, p. 355) reports that he has likewise found this to be true.

Fifth, the experiments in which the reactor held his finger on the tambour with every muscle of the hand and arm rigid and then reacted, showed by the presence of the antagonistic movement that it is no accidental fact of muscular twitching, but a significant physiological fact.

Our physiological explanation of the phenomenon would then be similar to that advanced by Smith (**5**, p. 57). Where the antagonistic movement occurs, the muscles of the hand and arm have been, in our spring form of apparatus, innervated to hold the key down. When the stimulus comes, a sudden reflex innervation takes place along the line of least resistance, namely, into the open motor tract leading to the extensor muscles. This increased innervation of the muscles already innervated causes the antagonistic movement. The voluntary innervation of the retractor muscles then occurs, involving the inhibition of the antagonistic extensors, and the upward reaction movement follows.

With this explanation it is easy to see why the antagonistic movement should occur in the sensory and not in the motor series. In the sensory reaction the muscles in the hand and arm are chiefly engaged in holding down the key and when the stimulus comes the additional innervation joins in with that already predominating and causes the sudden movement in the existing direction of pressure.

In the motor reaction, contrariwise, the muscular preparation is for a quick upward movement. In many cases this movement is already begun before the stimulus arrives. When the stimulus comes, the sudden reflex innervation finds, therefore, the line of least resistance not along the motor nerve tracts to the extensor muscles, since these are already partially or wholly closed but, rather, along the more open tracts to the retractor muscles. The added innervation, reinforcing that already dominant, causes the quick upward reaction movement. Whenever the attention is turned away from the preparation for the upward movement, it is probable that the innervation of the extensor muscles is again increased, for unless there is a continued and positive effort towards an upward movement, the sensations of the pressure of the key against the finger would doubtless cause a continuous innervation of the extensor muscles in reflex response to that pressure. In this way the antagonistic movement would also occur in motor series, at times of distraction or lowered attention.

Smith gives also a psychophysical explanation of the antagonistic reaction, which we have outlined on pp. 57-58. Judd, McAllister and Steele (**3**) have criticised this on the ground that "the sudden antagonistic movements are facts of nervous mechanism rather than facts of conscious expression." True, the antagonistic movement is a fact of nerve mechanism, but it is also a fact of conscious expression since, under certain conscious conditions, i. e., sensory, it does normally occur while under others, i. e., motor, it does not. Smith stated that in the period before the stimulus the mental and muscular preparation was chiefly engaged in holding down the key, and that often under such conditions the antagonistic movement would occur, for the reasons stated above. What this investigation has added is the fact that the mental and muscular preparation is *not always* engaged in holding down the key, namely, in the motor reactions, and that in such cases the antagonistic movement normally does *not occur*.

One other fact seems to be shown by our results, and that is that reaction time work which is done with the *press reaction* will be free from the complications due to the antagonistic movement.

We must not neglect to speak of any evidence on the problem of whether a reaction is a brain reflex or a voluntary activity (see, also p. 143). Many records occurring in the sensory series when the reactor's attention was decidedly wandering or distracted, show clearly that the reaction tends to *begin* without coincident voluntary decision. The stimulus causes a reflex "start." In these cases, however, the reaction movement is never completed rapidly unless the additional voluntary effort is added. When the attention is not brought back by the stimulus, the reaction completes itself very gradually after the first rapid start. When the attention *does* recur to the stimulus, it is brought back by a reflex start and then the much quicker and stronger voluntary reaction follows. The evidence for this is that the reactions following the return of a wandering attention to the stimulus, when it occurs, are not gradual, but sharp and quick.

If, furthermore, the theory presented to account for the antagonistic movement is a correct one, many sensory reactions would be excluded from the reflex type. The antagonistic movement itself is a reflex and in cases where this always occurred,—that is, in reactors typically sensory,—this preliminary antagonistic movement might give rise automatically to the final upward movement, so that the whole reaction would be reflex. In many cases, such as the foregoing cases of distraction or lowered attention, however, a voluntary upward movement seems necessary in addition to the reflex antagonistic movement caused by the stimulus. The preliminary "start" in such reactions seems allied to those slight partial reactions to the warning signal and to those same phenomena in the discrimination reactions in which the reactor made an involuntary movement at the wrong stimulus but did not complete the movement.

In the motor reactions, especially with reactor M., the facts appear to show an approach to the reflex. The reactor was often surprised that he had reacted and was not always sure that it was the stimulus that had set off the movement. In the period before the reaction both the character of the movement and the introspections show that a definite mental and nervous preparation had been going on,— a preparation to react as quickly as possible. The stimulus was the "last straw," so to speak, and the involuntary movement resulting occurred reflexly in the direction of the preparation. But this final reflex involves, as has been already emphasized (p. 143), the preparatory psychophysical "set." This agrees with those who claim that the motor reaction is a reflex act performed because of a pre-act of will.

My results show a definite correlation between the three factors, form of movement, conscious state, and reaction time. In discussing the different reactors we have shown the correlation between conscious states and forms of movement. We have shown that with each of the two typical forms of attention—attention on the stimulus and attention on the movement—there went, in the majority of cases, a definite form of reaction movement, the antagonistic reaction movement accompanying the sensory, and the sharp reaction, the motor attention. Our results have also shown a definite correlation between conscious states and reaction times, agreeing with the results of previous reaction time investigations. With the attention on the stimulus the times were longer; on the movement, shorter.

And, lastly the results have shown a correlation between forms of movement and reaction times. In the case of reactor M. with the attention fixed strongly on a muscular preparation for a quick movement, an upward preliminary movement resulted. This is undoubtedly best suited for a quick final reaction. The simple sharp reaction is also very favorable to a quick reaction time. Reactor H., whose attention was turned towards the coming stimulus, carried on at the same time an increasing muscular preparation in a direction opposite to that of the final movement; this preparation is also well fitted to give a rapid movement, but in a direction opposite to that required to complete the reaction. Hence, the antagonistic movement resulting from this type of preparation, although itself rapid, increases the total reaction time by 25σ to 30σ.

Have we not, in this fact of the preliminary increase in pressure followed by the antagonistic reaction in the sensory series and in the fact of the gradual upward curve followed by the sharp reaction of reactor M. and its explanation, come very close to explaining the difference in time between sensory and motor reactions? In speaking of the relation of the reaction movement to the reaction time, we do not refer, of course, to different forms of movement as influencing the time with no relation to consciousness; for time, form of movement, and conscious process all vary, in great degree, together.

One question in regard to the sensory and motor reactions we have not considered. Has this investigation shown why some reactors should give motor times nearly as long as, or even longer than, sensory? In the case of reactor H. the motor times were only 3σ shorter than the sensory times. But H. interpreted the instructions "to think of the movement" as meaning a concentration of the attention on the finger in an effort to hold it down. This fact caused, therefore, more marked antagonistic reactions in the motor than in the sensory series. Such a tendency may appear in the case of many reactors of the sensory type, and would account for a part of the longer times in their so-called motor reactions. The rest might be adequately accounted for by the distraction caused by the unusual direction of the attention.

This investigation has shown very clearly that there are individual types of reactors. Reactor M. adopted the motor attitude from the first. He liked the spring key because it allowed him to prepare for the final movement by raising the hand before the stimulus. He did not like the electric key, since this natural tendency to prepare for the final reaction movement could not be carried out without breaking the circuit. This fact changed his whole attitude, causing him to think of holding his finger down on the key, and inhibited the natural tendencies, increasing the reaction time nearly 50σ.

Reactor H., on the other hand, adopted the sensory form of attention from the first. He did not like either the spring key or the tambour, because they did not furnish a firm foundation from which to start. *Why* a firm foundation was needed H. could not tell. The reason was without doubt connected with the necessity of checking an antagonistic movement, already begun, in order to begin the upward movement. H., in contrast to M., liked the electric key because of the feeling of security that it gave. H. believed himself to be executing much faster reactions on this key, while in reality his reaction times were increased by nearly 30σ. The mean variations were decreased by one. The reason for this increase in time is not clear. It may have been connected with the fact, which H. recorded in his introspections, that this key required more pressure, or it may be that this form of key, in interfering with the extent of the possible antagonistic movement increased the time in some way. If this latter suggestion were true, then the error due to the antagonistic movement would be greater in the electric form of key than in a key which allowed for the natural movement, increasing, thus, the total reaction time. Reactor M. stated in one introspection that the electric key required more pressure and that more time was taken in overcoming this pressure. Such a statement may throw some light on the problem.

Of the other reactors, E. and W. were distinctly motor in type. As we have shown, the reactor's type is revealed by his attitude during the experiment, which influences both the form of movement and the time of reaction. With E. these all changed, showing that sometimes a type develops, even without instructions. The conditions at first may be so new and strange that the characteristic type is concealed. As adaptation to these sets in, the natural type emerges—in E.'s case, the motor type.

It would be interesting to follow the changes in the form of movement, in correlation with the attitude and the times, which accompany extended practice in both kinds of reactions. If it is true that the types first separate and then approach each other, what part does the form of movement play in this process?

Though quite definite individual characteristics appear in the pressure curves of the different types of reactions, this investigation has shown that the forms of movement are more closely connected with general attitudes and types of attention than with individual characteristics. In their earlier work Judd, McAllister and Steele found it possible to change the character of the movement at once by changing the character of the task required. This we also found to be true, not only with a change in apparatus, but when only a change in attention was involved; good examples of this may be found in the reactions of J. (Table XIV).

We likewise found, as in the earlier work, that different forms of movement suddenly made their appearance and are scattered through the series at random. We have been able to connect these with variations in attention. This being the case, it is manifestly in order to take graphic records of *all* reactions before anything of importance can be inferred from the times themselves; and as we have shown, even if the introspective evidence is perfectly clear and valid, many radical changes in movement go entirely unnoticed by the reactor. This is specially true in the sensory reactions where the attention is turned away from the movement.

Our results give little help to the experimenter who asks for some easy way of determining the type of attention or the form of the movement by a study of the mere reaction times. The mere fact that a reaction time is longer than the average does not prove that it is caused by an antagonistic movement; neither does the fact that it is shorter show the absence of an antagonistic movement.

Such results as ours show, finally, very clearly the need of subjects carefully trained in introspection with which to supplement the graphic records. If it is answered that psychology is as much interested in the untrained and unpracticed subject as in the trained subject, we agree, but in order definitely to settle this question of the correlation of forms of movement with conscious attitudes or with types of reactors, will it not be necessary *first* to use trained subjects, so that we can be sure that the form of movement studied really *did occur under the conditions imposed?* Such a preliminary study would make much easier the study of the untrained subject.

Future work should be, therefore, *first,* along the line of perfecting apparatus; and *secondly,* experimentation with subjects thoroughly trained in introspection and possessing definite characteristics.

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Vol. XVII
No. 5

PSYCHOLOGICAL REVIEW PUBLICATIONS

December, 1914
Whole No. 76

THE

Psychological Monographs

EDITED BY

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The Measurement of Attention

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PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
and LANCASTER, PA.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.);
Leipzig (Hospital St., 10); Paris (76 rue de Rennes)

PRINCETON
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CHAPTER I

INTRODUCTION

The problem of the measurement of the degree of attention has for some time been recognized by writers of the highest authority as one of the central problems of experimental psychology. There is, consequently, no need to dwell upon its importance. What is believed to be a satisfactory solution of this problem is offered in the following pages.

While the writer is of the opinion that an accurate and practicable method for the measurement of the degree of attention has not hitherto been described, it is only fair to acknowledge that many methods have been proposed. The reader who desires a critical historical study of methods of measurement of attention may be referred to Geissler.[1] In summary, Geissler writes: "We have thus completed our review of the experimental work upon the problem of measuring the attention and may conclude, on the one hand, that, although much effort has been spent upon it, we are still far from a satisfactory solution. On the other hand, we are now better able to judge which paths would seem most promising for future experiments and which should be abandoned."

As regards Geissler's own contribution, the main conclusion at which he arrives is the following: "A very close parallelism was found to exist between introspectively distinguishable variations of attention and corresponding differences in the precision of work performed at these levels, under the condition that the estimation of degrees of attention was made in terms of clearness and that the work itself was not influenced by anything else but change in attention." That a parallelism exists between degree of attention and various kinds of work has long been taken for granted. Indeed, it is hard to imagine any psychophysical work in which attention is a factor, the efficiency of which would not show variation when attention varied, and which would not accordingly show a parallelism with attention under the condition that "the work itself was not influenced by anything else but change in attention." But Geissler's work experimentally establishes the validity of this assumption.

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[1] *Amer. Journ. of Psychol.*, 1909, 473-529.

Concerning the bearing of this correlation between estimated degrees of clearness and efficiency of work upon the measurement of attention, Geissler writes: " . . . we believe that our results have brought us within reach of a new and fairly definite method of measuring attention, for the results have shown that degrees of clearness are just as accessible to introspective determination as variations of the intensity of sensations. . . . There is sufficient warrant in our results for the assumption that continued practice will lead at least to a differential clearness limen which may be just as definite as any other psychophysical limen, while on the other hand, it may be perhaps more difficult to establish the least possible or the highest possible clearness degree for any given mental process. However, the determination of a difference limen for clearness would be the most important step toward an exact measurement of the concentration of attention. It would enable one, by starting with a certain clearness degree of a given mental process under fixed experimental conditions, either to increase or decrease that clearness by just noticeable differences, until the maximum or the minimum of attention to the particular process is reached."[2]

Concerning this differential limen, which Geissler says would be the most important step toward an exact measurement of the degree of attention, all that can be said in reply is that no such limen for clearness has as yet actually been measured, and that Dallenbach[3] in his recent article which is avowedly a continuation of Geissler's work, does not even refer to the matter.

It is perhaps possible that, apart from the matter of a differential clearness limen, use may ultimately be made in the measurement of attention of the high correlation which both Geissler and Dallenbach have found to exist between efficiency and estimates of clearness. Neither of the authors mentioned, however, deny that efficiency may vary as the result of other factors than attention; nor do they explain any procedure for keeping other factors than attention constant, though their statement of parallelism between estimated clearness and efficiency is limited by the condition that no factor other than attention shall vary.

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[2] *Op. cit.,* 522-523.

[3] *Amer. Journ. of Psychol.*, 1913, 466-507.

Both Geissler and Dallenbach have paid so much attention to the matter of devising suitable distractors, that their work has considerable bearing upon the method proposed by Titchener. Titchener writes as follows: "If we knew, for instance, that a certain sensation may exist in ten different degrees of clearness: and if we had at our disposal ten stimuli which, introduced by way of distraction, would reduce this sensation from the corresponding degree of clearness to total obscurity: then we might calculate, from the effect of a particular distractor in the particular case, what fraction of maximal attention the observer was giving to the matter in hand. The method is cumbrous and difficult to work out; but the writer believes that it may, some day, be successfully applied."[4] Such a method evidently requires above everything else a set of constant and reliable distractors. Indeed, while the theory of the distraction method of measuring attention has, in the rough, been known for many years, the rub has always been in finding suitable distractors—or, since as a matter of fact only one, and not a set, is necessary, a suitable single distractor. A method of distraction suitable to the measurement of attention has not hitherto been devised. Geissler found that even the most complex distractors, after a few days' work, were unable to induce great variations of attention,[5] and Dallenbach writes: "In choosing the distractors, our ideal was that of Drew, 'to arrange a series of tasks of increasing degrees of complexity which should from the normal make ever greater demands on the mind until the attention should pass from a fully concentrated to a completely distracted state.' This is the principle laid down by Stumpf in his *Tonpsychologie,* and by Titchener in his *Psychology of Feeling and Attention.* The results show that we were not successful in obtaining such a series of graded distractors."[6]

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[4] *A Text-Book of Psychology,* 1909, 296. *Cf.* also his *The Psychology of Feeling and Attention,* 1908, 276-282.

[5] *Op. cit.,* 513.

A quite different method of measuring clearness is that followed by Wirth.[7] Wirth's method is exceedingly complicated and utilizes very elaborate apparatus. There exists serious doubt whether Wirth's method accomplishes its aim. Geissler, in a second review[8] of Wirth's method after a reply[9] by Wirth to his first criticism,[10] concludes by still maintaining that Wirth "has failed to solve his problem" [11] and Gruenbaum in a criticism of the work of A. Kaestner u. W. Wirth entitled *Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe der Reaktionsversuche,* concludes as follows: "Meiner Ansicht nach ist also weder der Vergleich der Resultate nach beiden Methoden [the reaction method and the differential brightness limen method] positiv ausgefallen, noch eine einwandsfreie Darstellung des Aufmerksamkeitsreliefs gelungen, noch die Durchfuehrung der Aufmerksamkeitsverteilung gesichert gewesen."[12]

Whether the main trouble with Wirth's method lies in his units of measurement or in his method of securing distributed attention, it is difficult to decide. Serious objections may certainly be made to both. Wirth's fundamental proposition, to limit myself to his first method, seems to be that the clearness of any point of the visual field with respect to intensity can be measured by determining, first, the intensive differential limen with maximal attention upon the point in question, and second, the same limen with less than maximal attention to the point in question, i. e., with distributed attention, and third, dividing the first mentioned limen by the second.

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[6] *Op. cit.,* 489.

[7] *Philos. Stud.,* 1902, 635-659. *Psychol. Stud.,* 1906, 30-88; A. Kaestner und W. Wirth, *Psychol. Stud.,* 1907, 361-392 and 1908, 139-200.

[8] *Amer. Journ. of Psychol.,* 1910, 151-156.

[9] *Psychol. Stud.,* 1909, 48-72.

[10] *Amer. Journ. of Psychol.,* 1909, 120-130.

[11] *Op. cit.,* 156.

[12] *Zeit. f. Psychol. u. Physiol.,* 1909, vol. 53, 102.

As for the third part of the procedure, I must confess that it seems to me the result of incorrect and confused reasoning. And while this part of Wirth's procedure is unjustified, it is impossible on the basis of the data he presents to outline any other which would give a correct result. In Chapter III it will be shown that a decrease in the favorableness of the conditions of attention, under certain conditions, exerts an absolute effect inversely proportional to the degree of attention, while the relative effect may remain the same. A theoretical explanation of this fact is there given. If such a law as this held under the conditions used by Wirth, one would have to use the difference between the normal and distraction limena, and not their quotient, as the measure of clearness. Wirth takes the quotient, but he has no data which prove the validity of this procedure. It is based on the hypothetical and unsound speculation that all the component factors involved in the discrimination of intensity would vary in the same direction as the result merely of a change in attention. Now either attention alone changed, in which case the non-attention factors involved in the process of discrimination did not change at all, or else other factors than attention varied and it would then be hopeless to attempt to get a measure of attention or clearness from the resulting change in the limen.

Moreover, it is very apparent that a measurement of clearness possessing any accuracy can not be obtained by such a method of securing distributed attention as that used by Wirth. If the clearness of different points in the field of vision is to be determined by Wirth's procedure, it is essential that throughout any set of measurements that are to be compared, the distribution of attention used to secure less than maximal attention shall remain constant. But the distribution was produced solely by the voluntary direction of the subject. It can not be said to have been brought under experimental control. We have no guarantee of its constancy, through a series of measurements, and consequently we can not make a valid comparison of the different measurements. The very great irregularity in the results obtained do not justify any faith in the reliability of such a method of securing a constant distribution of attention. That the method was altogether inefficient is indicated by the result that the clearness of points in the area "attended to" was about as often either high or low as that of points in the area "distracted from," and that no reliable difference existed even *on the average* in the clearness of the points in the areas "attended to" and in the areas "distracted from."

Passing now to the method developed in the following pages, I wish to raise a question which is bound to occur sooner or later and so may as well be considered at the start. How can one claim to have a method of measuring attention when psychologists are not entirely agreed as to what attention is? One may arbitrarily define attention, a procedure which has the advantage of definiteness, or one may argue that the method is valid for any of a number of reasonable and generally accepted definitions of attention. Since what attention is, is more or less of a speculation, I prefer the latter course. It will be assumed that attention is a psychophysical process, the intimate nature of which is still in doubt, but which is known by certain of its functions. By a function of attention is meant something which varies whenever attention varies. Electricity is perhaps a roughly analogous illustration of a process known only by its functions. One of the most commonly accepted functions of attention is clearness. I do not regard clearness as a synonym of attention but as a function of attention. Another function is limitation or narrowing of the field of consciousness. Another function is efficiency in any psychophysical process, that is, the greater the degree of attention, the greater the efficiency, other conditions remaining constant. There are many other functions. Attention may be satisfactorily defined in terms of any of its essential functions. The function by which attention is defined must always vary when attention varies, providing other conditions remain constant.

The function of attention which I have chosen for the purpose of measurement of the degree of attention is efficiency in a psychophysical act. There is no more important function. Efficiency, has, morever, been shown to correlate very highly with clearness by Geissler and by Dallenbach. It is true that the efficiency of any act is determined only in part by attention, but it is not here proposed to measure attention by simple efficiency. The efficiency utilized in the method described in the following pages is efficiency (quickness) in reaction. This particular efficiency has already been found by Dallenbach to correlate highly with clearness. He writes, "Attention may be measured introspectively in terms of attributive clearness. For introspectively distinguished variations of attention (i.e. clearness) are closely paralleled by corresponding differences at the same level in accuracy of work performed, in rate of reaction, and in degree of precision as expressed by the m.v."[13] Consequently, it seems to me that any one who holds any of the generally accepted views of attention can, if he so desires, relate his definition to the method of measurement here proposed in such a way as to allow the validity of the method. I might put the matter in this way: it is generally admitted that an increase in attention shortens reaction time, and I mean by attention whatever is usually meant by it in this statement. Again, intensity of stimulus is generally agreed upon as an objective condition of attention, and I mean whatever is meant by attention when one makes this statement.

A few pages may now be given to a discussion of the general principles involved in a measurement of attention by means of its effect upon, or expression in, efficiency. At the same time, as an orientation to the following chapters, it will be pointed out how these principles have been observed in the present work. In the first place, it is necessary to have some act the efficiency of which varies with the degree of attention. Since it is not necessary that this efficiency shall vary only with variation in attention, there is no difficulty here. Almost any psychophysical act varies with the degree of attention. The act I have chosen to use is the simple reaction. That reaction time varies with the degree of attention has long been known, and is demonstrated over and over again in each of the following chapters. This is a fundamental proposition in the theory of the method here proposed for the measurement of degree of attention, but the evidence in support of it[14] is so overwhelming that it is unnecessary to argue the matter. The fact that reaction time varies with attention does not make of it a satisfactory measure of attention. Reaction time varies as the result of variation in other factors than attention: and it is just this fact that makes it so desirable to devise a distraction method. A distractor must be found which acts on attention and on attention solely, one which lengthens reaction time solely by lessening the degree of attention involved in the reaction. After such a distractor is found it is still necessary to determine how the effect of such a distractor varies with variation in the degree of attention. The formulation of the relationship between the distracting effect of the dictractor and the degree of attention against which the distractor acts is a difficult and hitherto unsolved problem. Certain formulations might be arrived at which would not enable us to use the distraction effect as a measure of attention. A significant and useful formulation requires that the subject's efficiency be measured in units which fulfill certain conditions, as will be pointed out later.

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[13] *Op. cit.*, 507.

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[14] Obersteiner, *Brain*, 1879, 439-533.
Buccola, *Rivista di filos. scientif.*, I, 219 ff.
Hall, *Mind*, 1883, 170-176.
Cattell, *Mind*, 1886, 242.
James, *Principles of Psychology*, 1890, vol. I, 427-434.
Dwelshauvers, *Phil. Stud.*, 1891, 217-250.
Swift, *Amer. Journ. of Psychol.*, 1892, 1-19.
Bliss, *Studies from the Yale Psychological Laboratory*, 1893, 16-17.
Sharp, *Amer. Journ. of Psychol.*, 1899, 356.
Binet, *L'Année psychol.*, 1899, 275-282.
Ebbinghaus, *Grundzuege der Psychologie*, 1902, 589.
Wundt, *Grundz. d. physiol. Psychol.*, 5th ed., 1903, vol. III, 441 ff.
Toulouse, Vaschide et Piéron, *Technique de psychologie experimentale; examen des sujets*, 1904, 181-183.
Moore, *Psychol. Rev. Monog. Suppl.*, 1904, **6**, 31-40.
Roehrich, *L'Attention spontanée et volontaire*, 1907, 64.
Pillsbury, *Attention*, 1908, 83.
Kaestner u. Wirth, *Psychol. Stud.*, 1907, 361-392; 1908, 139-200.
Della Valle, *Psychol. Stud.*, 1908, 283.
Meunier et Vaschide, *La psychologie de l'attention*, 1910, 43-47.
Arnold, *Attention and Interest*, 1910, 54-56.
Breitwieser, *Archives of Psychol.*, 1911, **18**, Chap. III.
Grassi, *Zsch. f. Psychol.*, 1911, 46-72.
Dallenbach. *Amer. Journ. of Psychol.*, 1913, 465-507.

While I have hitherto spoken of distractors and distraction methods there is evidently need of somewhat similar but broader terms. The word distraction implies a tearing asunder of attention, part remaining on the objects attended to before the distractor appeared, and part going to the distractor. The current psychological usage of the term distraction implies not only a division of attention but attraction of attention to the distractor; a distractor is also an attractor. While this latter implication is perhaps not a necessary one, the idea of division or tearing asunder seems unavoidably connected with the term. Now a distractor of attention results in a lowering of the degree of attention, because one of the conditions determining the degree of attention is the number of things simultaneously attended to. A distractor, then, means an unfavorable state of one of the conditions of attention, namely, the number of objects simultaneously attended to. But there are many other conditions of attention than the number of things simultaneously attended to: for instance, intensity, familiarity, etc. An unfavorable state of any of these conditions results in a lowering of attention, as does the presence of distractors, but does not necessarily result in a division of attention, and cannot, therefore, be spoken of as a distractor. I propose to use the term "detractor," a term which retains the significance of distractor with respect to the effect of the latter on the degree of attention, without implying division of attention. A distractor lowers the degree of attention by tearing attention asunder. A detractor simply reduces the degree of attention,—no matter how, whether by distraction or otherwise. Distractors are also detractors. A detractor, therefore, is an unfavorable state of any of the conditions of attention. The effects of alcohol upon the nervous system, weak intensity of stimulus and fatigue or lack of interest on the part of the subject are illustrations of detractors. They are conditions which detract from the degree of attention.

In accordance with the terminology suggested above, the method of measuring attention described in the following pages would be called a detraction method rather than a distraction method. The latter term would be misleading. The theory of the method, however, remains in general the same as that of a distraction method, in spite of the slight change in terminology. The important point is that a detractor may lower the degree of attention without bringing about a state of divided attention.

The conditions with which a detractor of attention must comply, in order to be satisfactory for the purpose of measuring attention, have often been discussed, and I shall emphasize here only two of the most essential. The point of first importance is that the detractor shall act solely upon attention. The detraction effect which it produces (measurable in one of the functions of attention) must be due solely to its effect upon attention. Reaction or any other psychophysical act involves other factors than attention. If the detractor affects any non-attention factor it is unsatisfactory for use in the measurement of attention. The second most important condition is that the detractor shall be of such a nature that its magnitude as a detractor remains constant. A detractor which the subject can avoid or not as he will, to even a slight extent, will vary too much in its magnitude as a detractor to be satisfactory. Again if we find that its effect very rapidly wears off we can be sure that it is unsatisfactory. As a matter of fact, as will be shown later, especially in Chapter V, the effect of a suitable detractor remains approximately constant throughout a large number of successive applications, and we may lay down such a constancy as a test of a satisfactory detractor.

The attention that is measured by the detraction method described in the present article, is, as already stated, that involved in a simple reaction. Unfavorable preparatory intervals have been used as the detractor. On this account, my first task was to undertake a careful and extensive investigation of the effect of variation in the preparatory interval upon reaction time. This investigation constitutes Chapter II. The conclusion there reached is that the unfavorable preparatory intervals result in a prolongation of reaction time solely because of their effect as detractors of attention. Unfavorable preparatory intervals further recommend themselves for use as a detractor because they may be varied in unfavorableness and so graduated in magnitude as detractors if so desired, they are accurately measurable and accurately controllable, are unavoidable, do not involve divided attention, and as is pointed out more especially in Chapters III and V, are eminently constant in their effect upon a given attention as long as all other factors remain the same. They may be used to detract from attention in various situations, e.g. in sound reactions or light reactions, simple reactions or recognition reactions, etc., and they may be easily used simultaneously with other detractors.

Having come to the conclusion in Chapter II, that the unfavorable preparatory intervals constitute a satisfactory detractor of attention, I proceeded next to determine how the detraction effect varies with the degree of attention detracted from. This part of the investigation, which I regard as the most important part, is reported in Chapter III. It was necessary to have different degrees of attention, which could be relied upon to remain approximately constant throughout the investigation. I could not obtain these different degrees by the use of my detractor, as my object was to determine how the effect of the detractor varied when applied to various already existing degrees of attention. I might have used subjects of different age, some children and some adults, and indeed did so later (see Chapter V) by way of confirmation of conclusions, but I preferred to produce the different degrees of attention in adult subjects by means of artificially controlling the degree of attention. For this purpose I had resort to a second detractor, or rather, set of detractors, namely, weak intensities of stimulus. Such detractors would be unsatisfactory for the purpose of measuring attention, as they affect other factors than attention, but they condition with great constancy different degrees of attention. Intensity being a condition of attention, a decrease in intensity detracts from the degree of attention. The reaction time is lengthened partly as the result of this detraction from attention and partly as the result of other effects of the decrease in intensity. The important point, however, is that different degrees of attention are secured, a point experimentally demonstrated in Chapter III. Having thus secured different degrees of attention, I applied my detractor to each of these degrees. The most useful formulation of the results from the point of view of measurement of attention is the following law: The absolute increase in reaction time produced by the use of unfavorable intervals as a detractor varies inversely as the degree of attention. This law is arrived at again in Chapter IV and still again in Chapter V. In the work reported in these latter chapters, the different degrees of attention were produced in a variety of ways other than that described in Chapter III. A corollary of this law is that the degree of attention ranks as the reciprocal of the absolute increase in reaction time produced by the detractor. Simple reaction time may vary widely from time to time and from individual to individual without great variations in attention, because the non-attention factors may be different in different cases. In general, of course, some degree of correlation will exist between simple reaction time and degree of attention, but the fact that reaction time will vary as the result of other conditions than attention prevents our using the simple reaction time as a measure of attention. This is why it is necessary to apply a detractor of known magnitude, which acts solely on attention, and then observe the effect of this detractor; in short, to use a detraction method. And this method happens to be feasible because under proper conditions the effect of the detractor varies in an ascertainable way with the degree of attention, as expressed in the above mentioned law.

Whether the above law governing the relationship between the effect of a detractor upon reaction and the degree of attention detracted from can be generalized depends upon what one understands by an adequate measure of the detraction effect. I can, of course, not be sure how much general validity attaches to a relationship that I have found to exist under certain special conditions. The data of the following pages strongly indicate however the following general law of detraction: The absolute effect of a given detractor varies inversely with the degree of attention detracted from. I am inclined to believe that this law has general validity providing the detraction effect is adequately measured. If the detraction effect used is a lessening in efficiency of some sort, the efficiency must be one that varies very delicately with variation in attention. If the act is a reflex, we cannot use changes in it as a measure of the detraction effect. Moreover, the efficiency must be adequately measured. The average number of points per inning made by a billiard player during an hour may not adequately measure his efficiency. The difficulty of the shots, and the size of the misses, etc., would have to be considered. Another consideration of the greatest importance is that efficiency cannot be adequately measured by the amount done in a given time. Paradoxical as it may sound, the time required to do a given amount is a much more adequate and accurate measure than the amount done in a given time. In measurement by amount done, poor efficiency is not given sufficient chance to display itself. If a person is rather poor and so accomplishes only a small amount in a given time, there is, so to speak, lack of room for a great absolute decrease in this amount done, no matter how much worse the individual becomes. There is no limit on the other hand to the increase in time that he may require to accomplish a given amount of work. There is infinite room for the time to become longer. The general law of detractors that I have stated certainly would not hold if efficiency were measured by amount done in a given time. Suppose, e.g., that a normal individual did 10 units normally and 5 under detraction, in a given time of 10 secs., and that an imbecile, in the same time, did 5 units normally and 2 under detraction. If we called the decrease in amount done the absolute detraction effect, according to the above law the imbecile would have the better attention, as his decrease is only 3 units while the intelligent individual's is 5 units. On the other hand, assuming that the amount done varies in direct proportion to the time allotted, the normal individual would require 10 secs. to accomplish 10 units under normal conditions and 20 secs. under detraction, while our imbecile would require 20 secs. normally and 50 under detraction. If now we measure the detraction effect by the increase in time required to do a given amount of work, according to the law of detraction stated above the normal individual has much better attention, as his time under detraction increases only 10 secs., while that of the imbecile increases 30 secs. The latter procedure may confidently he said, on the basis of the data to follow, to be correct: and consequently it may be further stated that the detraction effect upon efficiency must be measured by the increase in time required to do a given amount (or the increase in average time per unit) or else by some method which complies with the same conditions as such a measurement. A time measurement has the characteristic of being minutely divisible, but much more important is the fact that there is no danger of the efficiency becoming so low or the detraction effect so great, but that a time measurement will always adequately reflect the existing degree of efficiency. Lastly, it must be remembered that a time measurement can not be a satisfactory measurement of efficiency except when the work is done with the sole idea of doing it as quickly as possible, as for example in the case of a "motor" reaction.

The attention measured is, of course, not the "general power of attention" of the individual in question, but the average degree of attention involved in his reactions. Not the attention he "might" give, but the attention actually present. An attempt has been made, however, to show that the measurement may be made independently of the factor of sensory sensitivity, in other words, to develop a method of measuring "attentions," whether of different individuals or of the same individual at various times and under various conditions, which would rank these "attentions" only in accordance with the way in which they differ because of other factors than sensory sensitivity. The results presented in Chapter IV show that this is possible, when we use as the reaction stimulus a change in intensity in place of a given absolute intensity. This is a point of importance in the comparison of different individuals, where considerable differences may exist in sensory sensitivity. It is clearly desirable to be able to compare individuals with respect to attention apart from sensory sensitivity, and to be able by the use of two methods, one which eliminates and one which does not eliminate sensory sensitivity, to determine the effect of sensory sensitivity on the degree of attention. The perfecting of a method of measuring attention independently of sensory sensitivity is also of importance in connection with determining whether individuals rank the same in auditory as in visual attention. Before we can say that the type or quality of mental activity is a condition of the degree of attention we must equate the intensity factor, and this can hardly be done in the case of sight and hearing except by devising methods of measurement which are independent of both intensity of stimulus and the degree of sensitivity of the sense organ. It is intended soon to publish an investigation of the degree of correlation between measurements, made by the same method, of degrees of attention in the same subject in the case of different modes of stimuli, but data on this question are not presented in the present work.

In Chapter V, I have presented the results of a rigorous test of the constancy of the unfavorable preparatory intervals as a detractor of attention under the heading of "Practice". The results obtained here lead to several very important conclusions. The effect of the detractor is shown not to decrease nor to greatly vary as it is used over and over again upon the same individual. This leads to the further very important conclusion that the degree of attention involved in the reaction experiment does not improve with moderate practice. A few further sets of measurement are also included in Chapter V, partly to illustrate the use of the method and partly to confirm conclusions arrived at in the preceding chapters.

While the present work is primarily a study in the measurement of attention, it is at the same time a rather elaborate study of reaction time. The total number of reaction times reported in the following pages is 30,000, taken usually in series of 30. The mere labor of computation of averages and mean variations has occupied many weeks. The particular conclusions arrived at concerning reaction time will be found in the summaries given at the close of each chapter.

CHAPTER II

THE EFFECT UPON REACTION TIME OF VARIATION IN THE PREPARATORY INTERVAL

While a number of investigators[1] have studied the effect upon reaction time of variation in the preparatory interval,[2] no one has carried out the investigation thoroughly and extensively enough to permit of a full understanding of the whole matter. Only two investigators, Breitwieser and Della Valle, have published systematic investigations on the effect of intervals extending up to, or beyond, 10 secs.[3] I do not dwell on the fact that these two authors give an entirely different picture of the effect of variation of the interval. Breitwieser's contribution is so much more thorough than Della Valle's, that it may, no doubt, be regarded as superseding the latter, even though Breitwieser did not use intervals longer than 10 secs. But from work I had done before Breitwieser's contribution appeared, I knew that by using other series of intervals than the one he used, in the case where the intervals are mixed in irregular order, and by varying the order, I could get results which would greatly change the interpretation one might put upon results obtained from any single set of intervals. As an illustration of what I mean, Breitwieser, using intervals varying in duration, by steps of 1 sec., from 1 to 10 secs., mixed irregularly, found that "an interval of 2 or 3 seconds appears to be in general the most favorable," that is, gives the shortest reaction times. I had already found, using a set of intervals, varying, by steps of 0.5 secs., from 3 to 7 secs., mixed in irregular order, that the interval of 3 secs. was strikingly the least favorable, and in a series varying from 4 to 20 secs., by steps of 4 secs., that the interval of 4 secs. was the least favorable. Remembering that Breitwieser found the average reaction time of 18 subjects only 1σ shorter with a 2 sec. interval than with a 7 sec. interval, it may be said with great certainty that we both obtained perfectly normal and correct results, and that the results of the present investigation are in concordance with those of Breitwieser. Yet on the surface there is enough apparent contradiction to make it imperative to go into the matter more thoroughly.

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[1] Dwelshauvers, G., "Untersuchungen zur Mechanik der activen Aufmerksamkeit," *Phil. Stud.*, 1891, 217-250.

Moore, T. V., "A Study of Reaction Time and Movement," *Psych. Rev. Monog. Suppl.*, 1904, **6**.

Della Valle, "Der Einfluss der Erwartungszeit auf die Reaktionsvorgänge," *Psychol. Stud.*, 1907, **3**, 294-298.

Breitwieser, J. V., "Attention and Movement in Reaction Time," *Archives of Psychol.*, 1911, **18**, 18-38.

The writer, also, reported some results of a preliminary study of this subject some years ago. *J. of Phil., Psychol. and Sci. Meth.*, 1908, **15**, 413.

[2] By "preparatory" interval, I mean, of course, the interval elapsing between a warning signal of some sort, i.e., a signal to get ready, and the stimulus to which the subjects reacts.

[3] Dwelshauvers, in studying the effect of *omission* of a preparatory signal, used as the intervals between reactions 30, 45 and 60 secs.

Moreover, it seems to me that not enough attention has been paid to the different procedures that one may use in studying the effect of variation in interval upon reaction time. It is of fundamental importance to distinguish sharply between that procedure where the same interval is used a number of times in succession, and that in which a number of intervals are mixed irregularly. Some authors have used one of these methods and some, the other. It is true that Breitwieser, though confining himself chiefly to that procedure in which the intervals are mixed irregularly, to some extent used both procedures, but his investigation is not planned with the view of making a careful comparison between the two procedures.

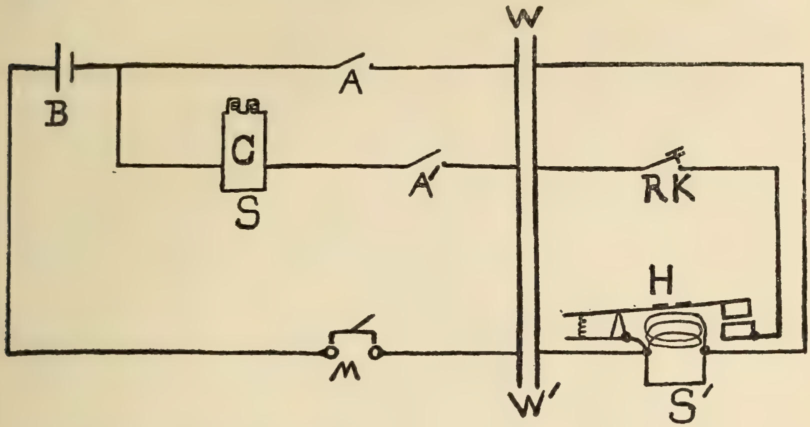
I shall first present the data obtained by using the same preparatory interval a number of times in succession, beginning with the shortest one and passing in regular order up to the longest. I shall reserve the comparison of the results obtained by different procedures until later in this chapter.

The apparatus for these reactions, which were simple, "motor", sound reactions, consisted primarily in a Wundt's sound hammer arranged in circuit with a Hipp's chronoscope and a Scripture's reaction key, the reaction movement being an upward one which broke the circuit. The source of current was a battery of Edison primary cells of about 10 volts. The chronoscope was controlled before each hour's sitting by means of a Wundt's fall-hammer, which in turn was checked about once a week by means of a 250 d.v. tuning-fork. The control was effected partly by regulation of the springs of the chronoscope, but chiefly by means of a slide resistance connected in parallel with the chronoscope. The fall-hammer was adjusted to occupy about 175σ from the "make" to the "break." I found, for the average of ten trials, the mean variation of the time during which the fall-hammer kept the circuit closed, to be well under 1σ, usually about .3σ or .4σ for an average time of 175σ. This, of course, includes the errors due to reading the tuning-fork records. The chronoscope was adjusted at the beginning of each sitting until the constant error for ten readings was not over 1σ (with a mean variation on the average of from 1 to 3σ). I took great pains with this adjustment, and would give up a sitting rather than proceed when the chronoscope was not entirely satisfactory in its readings. The preparatory intervals, which varied from 1 to 32 secs., were regulated by means of a metronome which could be set to make a circuit at any rate from once every second up to once every 7 secs. For intervals longer than 7 secs., I kept the circuit open by means of a key during certain "makes" of the metronome, that is, cut out either the alternate "make" or two or more successive "makes" of the metronome. This was easy for the experimenter to do, since the metronome made a different sort of a sound on the beat just preceding that which brought about the close of the circuit. The beats were audible to the experimenter owing to the nearness of the metronome, which, however, because of being enclosed in a chest, was absolutely inaudible in the neighboring subject's room.

The following diagram shows the disposition of apparatus: the fall-hammer, and the tuning fork, signal magnet and kymograph used in its control, are omitted. In actual work, however, the fall-hammer was never taken out of the circuit.

FIGURE 1

Apparatus for Sound Reactions with Controllable Interval between Signal and Stimulus.



A and A′ = switches, operated by experimenter.
B = storage battery.
C = Hipp's chronoscope.
S and S′ = slide resistances, used as shunts.
M = metronome, with two mercury cups, the points suspended from the cross-bar entering both cups simultaneously.
W W′ = wall separating experimenter's room from subject's room.
R K = subject's reaction key.
H = Wundt's sound-hammer.

After starting the chronoscope, with both switches, A and A′, open, the experimenter closed switch A at the moment when the circuit was open at the metronome. Switch A being closed, the next beat of the metronome would close the circuit passing through the metronome and sound-hammer, and so, as the result of the action of the sound-hammer, produce the warning signal. Switch A′ being open, the hands of the chronoscope would not start, whether the reaction key was closed or not. When the warning signal had been given, which the experimenter could determine by the sound of the metronome, leaving switch A closed, the experimenter promptly closed switch A′ also. Thus, when the metronome "make" occurred again, the resulting action of the sound-hammer produced the sound which served as the reaction stimulus and at the same instant closed the chronoscope circuit. The metronome circuit closed by the metronome "make" remained closed for over 800σ, so there was no danger of the circuit opening at the metronome before the subject had reacted. As soon as the hands of the chronoscope stopped, the experimenter quickly opened both switches and took the reading. In this way the duration of the preparatory intervals was determined accurately, the intervals being given with just the accuracy of the metronome. The intensity of sound used was moderate and remained the same throughout.

Three subjects were used, two advanced students, Sz and Vs, and the writer. The two advanced students had had a moderate amount of practice in reaction experiments at the time of this work, and this practice had included reactions with a wide range of preparatory intervals given in irregular order. They were not especially habituated to a 2 sec. interval. The third subject, the writer, had had a very great amount of practice in a great variety of reactions, usually with a 2 sec. preparatory interval. The reactions were "motor" or "abbreviated" reactions, and all the subjects had previously had practice in this form of reaction. As shown in the diagram of apparatus, the experimenter and most of the apparatus were in one room, while the subject sat in an adjoining, comparatively sound-proof room (this room happened to be a dark-room, but was artificially illuminated).

My reason for choosing the "motor" form of reaction and using it exclusively in the work reported in this monograph, is that I am convinced that the instructions for a sensory reaction are ambiguous, at least when it is held unpermissible in instructing for a sensory reaction to say "React as quickly as possible," on the ground that to react as quickly as possible is to use the motor form of reaction. If the instructions say "Put all attention upon the stimulus," the subject may really succeed and forget to react altogether. What actually happens, therefore, usually, is that the subject attends predominantly to the stimulus, but yet keeps his attention on the reaction enough to enable him to react promptly. But how promptly? Not as promptly as possible, and consequently as slowly and leisurely as he pleases, except in so far as the experimenter gives some incidental suggestion. Breitwieser's remarks are to the point:

"It is practically certain that the attitudes induced by different experimenters, under the name of sensory reaction, have differed. This difference need not have been due to the formal instructions, but the experimenter's own attitude and expectations may have been subtly imparted to his subjects. An exact definition of the sensory attitude cannot, therefore, be expected, but only an indication of some scale along which the attitude may vary. . . . Our results, therefore, suggest that, whereas the motor attitude is a single-minded preparation to react, the sensory attitude is a preparation to observe as well as to react, and that the preparation to observe may interfere in different degrees with the preparation to react, according to the amount of energy which is diverted from the preparation to react into the preparation to observe. In other words, the distribution of attention or of preparatory tension or innervation, varies in an uncontrolled way in the sensory type of reaction."[4] Moreover, as has been emphasized by Ach[5] and others, the sensory form of reaction tends to change with practice into the motor form. Ach also points out that the main distinction between the different forms of reaction depends upon the degree of determination to react as quickly as possible.[6]

The subjects were instructed to react always as quickly as possible, as soon as the stimulus sound occurred. I also emphasized with them that they were to try to do their very best every reaction and were neither to try to do exceptionally well any particular reaction or series of reactions, nor to let up at any time and neglect to react as quickly as possible. The reaction movement was always made in very much the same way. The subject placed the middle and forefinger of his right hand on the key at the warning signal, in case they were not already there, and held the key down until the reaction. If the subject at any reaction was not ready or distracted in any way, he signalled at the time to the experimenter and the reaction time obtained was thrown out, whether long or short. The subjects were instructed not to count, as I was afraid they might adopt this method of estimating the longer intervals. Just what difference it would have made, if they had, I do not know. All the subjects reported that they avoided counting.

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[4] Breitwieser, *Op. cit.*, p. 13.

[5] N. Ach, *Willenstätigkeit und Denken*, 1905, p. 108 ff.

[6] "Das Verhalten der Versuchspersonen bei den versichiedenen Reaktionsformen zeigt, dass nicht die Einstellung der Aufmerksamkeit auf den kommenden Reiz oder auf die auszuführende Bewegung das Wesentliche ist, sondern vielmehr der Umstand, wie sich Versuchspersonen bei ihrer Absicht zu den oben (S. 115) gegebenen Aufgabestellungen verhält, also ob sie z. B. die intensive Absicht hat, möglichst rasch zu reagieren (muskuläre-verkürzte Form) oder zu reagieren, nachdem sie den Sinnesreiz vollständig erfasst hat (sensorielle-verlängerte Form). Dabei ist es von untergeordneter Bedeutung wie diese Einstellung anschaulich in Bewusstsein repräsentiert ist, ob sie visuell, durch Fixation der Verschlussplatte, durch inneres Sprechen, durch intentionale Bewegungsempfindungen im reagirenden Muskelorgan oder in anderen Organen, z. B. in den Augenmuskeln u. dergl., gegeben ist." *Op. cit.*, p. 122.

The subjects were informed of the general nature of the experiment, and knew also the duration of the intervals used and the order in which they were to occur. They were informed that each interval would be repeated 25 times before the next longer one was taken up. They were not called upon to introspect more than to be sure that they followed the instructions. I did not wish them to try to introspectively describe the "action" consciousness. This was not my problem, and I did not want to require anything of the subjects which might distract them from the main task in hand, which was to react as quickly as possible every time the stimulus sound occurred.

The sittings lasted about an hour or sometimes a little longer, and occurred at the same time upon immediately successive days for each subject. Each "Series" in Table I refers to one hour's results of one subject. Each hour's work was as follows: After the control of the chronoscope, 25 reactions were taken with a 1 sec. preparatory interval; then 25 with a 2 sec. interval; then 25 with a 4 sec. interval, and so on up by steps of 4 secs. through a 24 sec. interval, and in some cases through a 32 sec. interval. The return to a 1 sec. interval, as though to repeat the whole experiment, was never made at the same sitting.

In presenting the results in Table I, I have followed the most widely used procedure, and given the average and the mean variation. In taking the averages no results were thrown out merely because they deviated greatly from the average. I may have included in this way a number of mistakes, but the only readings which I threw out (and I threw them all out) were those where I had noted possible error or mistake at the time, or those which were accompanied by a signal from the subject that he was distracted or not ready. In first working up my results

TABLE I

Effect of Variation in Preparatory Interval, Regular Procedure.
N, for each interval of each series = 25. Total number of reactions = 3,825.

Interval		1	2	4	8	12	16	20	24	28	32
Subject	Series I	159	148	159	198	227	217	240	267	...	...
Sz		*26*	*25*	*16*	*25*	*24*	*18*	*25*	*27*	...	...
“	“ II	140	128	147	173	188	200	200	235	...	...
“		*18*	*17*	*14*	*15*	*15*	*12*	*14*	*20*	...	...
“	“ III	140	118	155	166	185	190	209	208	...	...
“		*14*	*12*	*15*	*17*	*17*	*21*	*22*	*25*	...	...
“	“ IV	141	125	149	162	182	192	205	208	...	...
“		*16*	*18*	*14*	*15*	*16*	*17*	*20*	*31*	...	...
“	“ V	115	125	153	171	178	187	205	220	...	...
“		*23*	*24*	*14*	*17*	*18*	*20*	*20*	*24*	...	...
“	“ VI	100	123	157	194	194	216	217	220	...	...
“		*19*	*24*	*17*	*15*	*14*	*29*	*20*	*17*	...	...
“	Average	**133**	**128**	**154**	**177**	**192**	**200**	**213**	**226**	...	...
“	Av. M.V.	*19*	*20*	*15*	*17*	*17*	*20*	*20*	*24*	...	...
Ww	Series I	144	149	142	152	158	172	173	175	...	...
“		*21*	*19*	*12*	*12*	*14*	*17*	*13*	*24*	...	...
“	“ II	118	111	111	115	133	138	138	136	...	...
“		*14*	*12*	*13*	*13*	*12*	*18*	*13*	*12*	...	...
“	“ III	161	154	154	183	186	183	200	200	...	...
“		*15*	*11*	*11*	*29*	*18*	*20*	*18*	*21*	...	...
“	“ IV	171	158	156	183	198	190	198	195	...	...
“		*16*	*11*	*9*	*17*	*15*	*16*	*16*	*17*	...	...
“	“ V	162	138	151	156	162	166	167	171	...	...
“		*19*	*14*	*12*	*11*	*12*	*18*	*14*	*13*	...	...
“	Average	**151**	**142**	**143**	**158**	**167**	**170**	**175**	**175**	...	...
“	Av. M.V.	*17*	*13*	*11*	*16*	*14*	*18*	*15*	*17*	...	...
Vs	Series I	169	159	168	180	199	225	240	226	216	220
“		*18*	*13*	*21*	*18*	*13*	*40*	*39*	*40*	*42*	*33*
“	“ II	157	146	158	176	194	199	203	219	219	216
“		*16*	*10*	*16*	*23*	*21*	*41*	*23*	*39*	*35*	*30*
“	“ III	153	136	139	162	176	191	191	192	192	197
“		*19*	*9*	*14*	*12*	*13*	*18*	*12*	*14*	*15*	*21*
“	“ IV	136	114	140	153	169	177	181	184	199	193
“		*11*	*10*	*13*	*14*	*18*	*19*	*22*	*21*	*20*	*16*
“	“ V	143	148	160	186	189	198	202	204	218	216
“		*15*	*9*	*15*	*13*	*19*	*15*	*12*	*13*	*27*	*40*
“	“ VI	141	133	144	159	175	187	197	201	204	207
“		*11*	*7*	*11*	*8*	*12*	*10*	*26*	*16*	*19*	*15*
“	Average	**150**	**139**	**152**	**169**	**184**	**196**	**202**	**204**	**208**	**208**
“	Av. M.V.	*15*	*10*	*15*	*15*	*16*	*24*	*22*	*24*	*26*	*26*
Average for all subjects		**145**	**136**	**150**	**168**	**181**	**189**	**197**	**202**	...	...

I applied Chauvenet's criterion[7] for the rejection of extreme cases, but found it too laborious in view of the fact that its application did not seem to change the general trend of the results. It may be that I have included a number of prematurely begun reactions in the case of the 1 and 2 sec. intervals, but as these intervals were repeated only 25 times, I doubt if many such occurred. Of course a reaction which occurred before the stimulus would not be measured by the apparatus used.

The mean variation is given separately for each day's results. The "Series," each of which represents the data of one day's sitting, are arranged for each subject from top to bottom in the order taken. The average reaction time in thousandths of a second is given first, and the mean variation from each average given immediately below that average in italics. In the lines headed at the left, "Average," is given the average of all the average reaction times of each day; that is, the grand or total average. In the lines below these, headed "Av. M. V.," is given the average of the mean variations of the daily averages. The intervals are given in seconds in the first line of the table.

The data of Table I are represented graphically in Figure 2. The duration of the preparatory intervals is represented by the abscissae and the reaction time by the ordinates. Only the grand average reaction-time for each interval, for each of the three subjects is plotted.

Since the general course of the curve is somewhat similar for each of the three subjects, I have plotted a curve, Figure 3, which represents in graphic form the data for the three subjects regarded as constituting a single group, allowing equal weight to the results obtained with each subject. The numerical data represented graphically in Figure 3 are given in the bottom line of Table I.

That the averages and mean variations given in Table I truly represent the general tendency is shown by comparison with the full distributions of the last 100 reactions to each interval for subject Vs, presented in Table II.

Subject Vs is chosen as typical, and only the last four sittings with each interval are represented, so as to have data which do not represent too widely differing stages of practice. Even then, it must be remembered that each of these four sittings occurred on different days, and since the subject reacts uniformly faster or slower on any given day, a table which mixes the distributions obtained on different days gives a much less narrow range of reaction times for each interval than a table showing the same number of reactions all made the same day. On the

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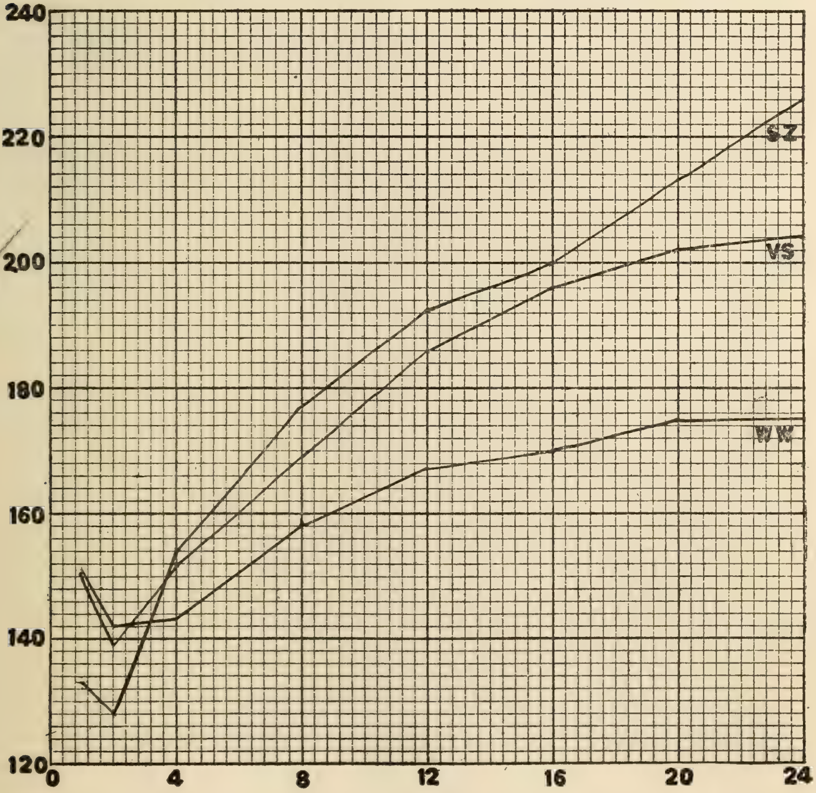
[7] See Merriman, *Method of Least Squares*, 1903, 166.

FIGURE 2

Graphic Representation of Effect of Variation in Preparatory Interval, Regular Procedure, for Each of Three Subjects.

The durations of the preparatory intervals are represented as abscissae; the reaction times, as ordinates. SZ, VS and WW stand for the three different subjects, for each of which the curve is plotted separately.



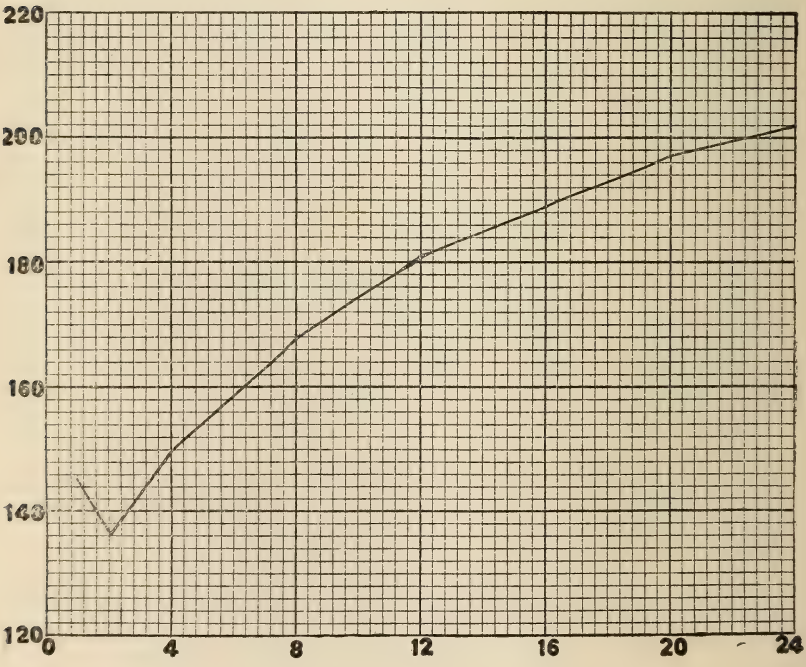
other hand, the *average* is probably all the more reliable, as any peculiarities in one day's results would tend to be eliminated by averaging with results obtained the other days. It is because the results for each day represent different stages of practice that I have not given in Table I the M. V. for the whole number of reactions, but instead, the average of the M.V.'s obtained on separate days.

The conclusions to be drawn from the data of Table I are quite simple, and clearly expressed in the curves of Figures 2 and 3. It must be constantly remembered that the conclusions hold only for the particular procedure used. Data presented later

FIGURE 3

The Curve Obtained by Combining the Three Curves of Figure 2.

The durations of the preparatory intervals are represented as abscissae; the reaction times, as ordinates. The formula for the above experimental curve is $y = A + B. \log x$, in which y equals reaction time and x the duration of the interval, and A and B are constants.



will clearly prove this. The interval of 2 secs. is clearly the most favorable. Subject Ww, however, the writer, who in general was the least affected by the duration of the interval, did practically just as well with an interval of 4 secs. The interval of 1 sec. and, likewise, any increase in the length of interval beyond 4 secs., is definitely unfavorable for all subjects, so that either decrease in the length of the interval to below the most favorable, or increase in its length beyond the most favorable interval, or range of intervals, results uniformly in longer reaction times. The curves of Fig. 2 are, in general, fairly regular, especially when we consider the kind of curves usually obtained in investigations of this sort. They present no well defined inversions and agree roughly with each other in form. It will be observed, however, that the subject who gives the quickest reaction time with a preparatory interval of 2 secs.

Table II

Effect of Interval, Regular Procedure, Shown by Complete Distributions of the Reaction Times.

Subject Vs.

Interval	2	4	8	12	16	20	24	28
R. T.								
.30 secs.								1
.29 "								
.28 "							1	
.27 "		1						
.26 "				1	1		1	1
.25 "							2	2
.24 "						1	1	4
.23 "					3	5	1	2
.22 "			2	3	3	19	6	8
.21 "			1	5	10	19	11	17
.20 "		1	4	20	18	18	22	18
.19 "	1	1	6	15	23	18	23	16
.18 "		2	18	23	21	11	16	16
.17 "		7	14	19	9	5	11	9
.16 "	7	17	23	18	9	1	2	2
.15 "	17	16	20	9	1	2	2	1
.14 "	27	23	6	2		1	1	2
.13 "	20	19	5		1			1
.12 "	16	9	1					
.11 "	9	3						
.10 "	1	1						
.09 "	1							
.08 "	1							
Total N	100	100	100	100	100	100	100	100

does not by any means give the quickest with the longer intervals. Indeed, for these three subjects there is an inverse correlation between the reaction time with the 2 sec. interval and that with the longer intervals. That the inverse correlation in the present case is perfect, is not of any real significance, owing to the small number of subjects. The only reliable point, and one of importance, is that a person who reacts relatively quickly with a 2 sec. interval may react relatively slowly with a longer interval, and vice versa.

The curve of Fig. 3 shows the general tendency of all the subjects, and its high degree of regularity shows that the effect of the preparatory interval on reaction time is far from haphazard. Both the general tendency and the tendency in each individual case is for the reaction time to increase at first comparatively rapidly and later comparatively slowly with [illegible] increase in the duration of the preparatory inter[illegible] most favorable range of intervals. By the most [illegible] of intervals, I mean those near 2 secs. While 2 [illegible] favorable interval of any I used, yet undoubtedly intervals very near 2 secs. (extending even up to 4 secs. with subject Ww) would also give very short times, so that the statement that the reaction time increases at first rapidly with increase in duration of interval, does not hold until after not only the one most favorable point is passed, but until after a certain narrow range of very favorable intervals is passed. The mean variations show nothing particularly striking. They run about 10% of the average, and in general increase or decrease in absolute size in accordance with the average.

Since the results with each of the three subjects conform to the same general statement of the effect of the duration of the preparatory interval, I have deemed it worth while to state more definitely the general tendency of the group, as represented in Fig. 3. The mathematical formula for the simplest smooth curve that comes near fitting the experimental curve is $y = A + B. \log x$ in which y equals reaction time and x the duration of the preparatory interval, and A and B are constants. Calculating the reaction time by taking A as 117σ and B as 60σ and using common logarithms, the following comparison may be made of the calculated and the experimentally obtained values. The differences given are obtained by subtracting the calculated from the obtained values. R. T. stands for reaction time (expressed in σ).

TABLE III

Interval (x)	Calculated R. T. (y)	Obtained R. T. (y)	Difference
2 secs.	135	136	+1
4 "	153	150	−3
8 "	171	168	−3
12 "	182	181	−1
16 "	189	189	0
20 "	195	197	+2
24 "	200	202	+2

In view of the close correspondence between the calculated and obtained values it seems safe to state the effect of the intervals longer than the most favorable intervals, upon reaction time, when the above regular procedure is used, as follows: *The reaction time equals a constant plus the product of the logarithm of the interval and a constant.*

From this law it is evident that at least two factors are involved in reaction time. One of these, *A,* is a large constant, in the present instance 117σ. The other, *B. log x,* is an *additional* factor, which increases in size directly as the logarithm of the preparatory interval. Perhaps the constant, *A,* may be regarded as including certain specific psychophysiological factors which could not very well be supposed to vary with variation in interval, and the second factor, *B . log x,* as some more general factor including the attention factor, since, as will be pointed out later, attention certainly varies with prolongation in interval.

I have often confirmed the results presented above, as well as the results obtained with the irregular procedure presented later, in a general way, with students in my class in experimental psychology. As these students used a Vernier chronoscope, reading only to 0.02 secs., I do not feel justified in introducing these results as accurate scientific data, but mention the fact merely as confirmatory of the general correctness of the preceding results. I shall, however, present at this point other results which, though taken by a somewhat different method, definitely confirm the data of Table I, and on subjects not represented in that table, two advanced graduate students with a very great amount of practice in reaction time.

The method employed differed from the 'regular' procedure used for the data of Table I in that no particular warning signal was given. The subject was required to react to every sound of the series, the sounds being repeated at a definite rate, so that the subject reacted to each of a series of regularly recurrent sounds.[8] Each sound thus served both as stimulus and as warning signal for the succeeding sound. As there was not time to read a chronoscope between successive reactions, the graphic method was used, with a long-roll kymograph and a 100 d.v. tuning fork. My primary purpose in taking these reactions was to determine whether or not, when the series of sounds was run off regularly in this way, the attention would become adapted to the interval, and if so, for what range of intervals. For example, I wished to see whether the attention could be so adapted to a 5 sec. interval and each sound occur so exactly at the time it was expected, that the reactions would be as short as with the 2 sec. interval. Such a possibility seemed to be suggested by a number of writers on attention and rhythm, and especially by Schumann, in his writings on the perception of time. Schumann, in a discussion of the experiences of surprise and of strain of expectation, which are felt upon listening to a metronome which is beating at "fast" or "slow" rates, writes as follows: "Diese Nebeneindrucke sind jedoch bei den ersten Schlägen einer neuen Schlagfolge subjektiv deutlich. Nach kurzer Zeit passt sich innerhalb gewissen Grenzen die Aufmerksamkeit dem neuen Intervall an, die Spannung der Erwartung, bezw. der Nebeneindruck der Überraschung nimmt immer mehr ab, und jeder Schall wird schliesslich wieder gerade in dem Augenblicke erwartet, in welchem er eintritt."[9] Further, "Die bisherigen Auseinandersetzungen gelten übrigens nur für Intervalle, welche 2 Sek. nicht wesentlich überschreiten. Mit der Zunahme der Intervalle tritt die Einstellung der Aufmerksamkeit immer schwerer ein und die Vergleichung der intervalle wird entsprechend unsicherer."[10]

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[8] No data on reaction time with this procedure, so far as I know, have hitherto been published. Binet, however, in a review of reaction technique (*L'Année Psychologique,* 1895, 775), mentions the fact that he used this method in collaboration with MM. Philippe et Courtier.

"Ce dispositif expérimental permet de noter que differentes personnes peuvent aller jusqu 'à des limites differentes pour les intervalles entre deux réactions successives; ainsi la plupart peuvent réagir lorsque le coup de marteau arrive toutes les deux secondes; lorsqu 'il arrive toutes les secondes il est déjà très difficile de réagir à chaque coup, on a une tendance très forte à soulever le doigt simultanement avec le coup du marteau, c'est à dire, à faire des mouvements rhythmiques correspondant aux coups de marteau, il faut exercer un effort d'attention très considérable pour arriver à des reactions aussi rapides; la plupart des personnes échouent apres une dizàine de reactions faites de cette sorte; de plus, les durées des reáctions varient beaucoup et peuvent indiquer le degré d'attention soutenue, et aussi la fatigue.

Quelques personnes puivent faire de bonnes réactions, nullement anticipées, avec intervalles d'une demi-seconde. . . ." *Loc. cit.*

I sought to obtain an objective test of the validity of these statements by means of reaction time. Since, when the adaptation of attention to the rate of succession of the sounds is perfect, each sound occurs exactly when it is expected, within the range of perfect adaptation there should be no variation in the average reaction time of reactions made to every sound of the series; and if this adaptation of attention becomes constantly less perfect as the rate is made slower than one sound every 2 secs., the reaction time should become constantly longer and the increase in reaction time should measure the lessening in the degree of adaptation of attention.

The subjects were instructed to try to get into the tempo, and the first few reactions were not recorded, the idea being to wait until the subjects were adapted to the rate. I was unable to obtain reactions at a rate faster than one every 2 secs., as with very fast rates it becomes very difficult to keep from beating time instead of reacting. The subjects were instructed to use the "motor" form of reaction. The intensity of the sound stimulus was rather weak in the case of one subject, Hh, and very weak in the case of the other, Br. This explains why the reaction times are so long and why the times for Br. are so much longer than for Hh. The idea in using the very weak stimulus, as with Br., was that with a very weak intensity of stimulus, the degree of adaptation of attention would be a relatively more important factor in determining the reaction time, and that consequently variations in the degree of adaptation could be more readily detected. That this surmise was correct is indicated by the marked prolongation in reaction time which occurs with the longer intervals. It is also very clearly proved by the data presented in the next chapter.

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[9] *Über die Schätzung kleiner Zeitgrössen, Ztscft. fur Psychol. und Physiol.,* 1893, **4**, 3.

[10] *Op. cit.,* 7.

TABLE IV

Effect of Variation of Rate on Reactions to Each of a Series of Regularly Recurring Sounds.

N, for each interval, for subject Br, = 75, for Hh, = 30.

The average reaction time for each rate is given in σ. The mean variations are given in italics just below the corresponding averages. The rate, i.e., the interval between successive sounds of a series, is given in secs. in the first line of the table, headed "Interval."

Interval	2	3	3.5	4	5	6	7	8	10	12
Subject Br	198	214		243	260	260		289		299
	31	*19*		*28*	*30*	*31*		*25*		*30*
Subject Hh ...	164		158		172		214		230	
	17		*17*		*25*		*39*		*22*	

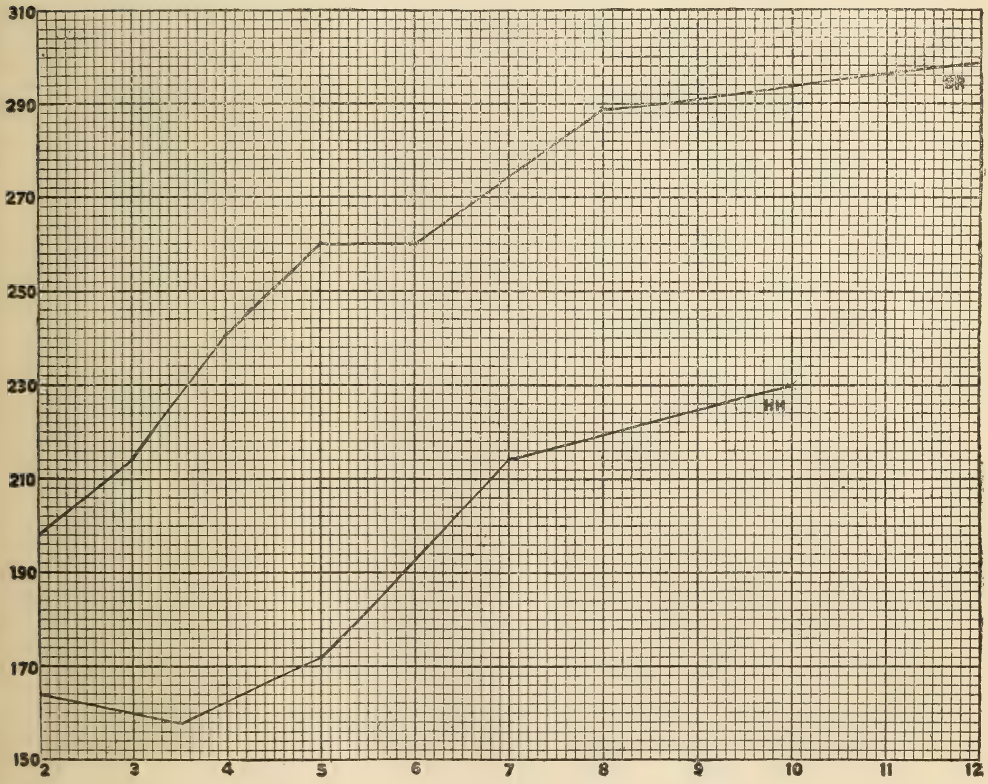
Average reaction time of 100 reactions, subject Hh, with intervals from 1 to 6 secs. inclusive, mixed in irregular order, 234 σ, M.V. 28 σ; for 25 reactions with intervals from 7 to 20 secs. irregularly mixed, 258 σ, M.V. 21 σ.

The results with this procedure are given in Table IV and Fig. 4. It is evident that there exists no great degree of flexibility in the range of rates to which the attention can become perfectly or maximally adapted. The curves begin their rise with quite short intervals, and rise rapidly, indicating that perfect adaptation cannot be secured with rates slower than one sound every 4 secs. They are not so regular as the curves of Figs. 2 and 3. This is in part due, no doubt, to the smaller number of reactions represented in Table IV, but more largely, I think, to the fact that, in the experiments represented in Table IV, I did not proceed in regular order from the fastest rates up to the slowest, but in a rather irregular order, of which unfortunately I did not keep account. Considerable time, 10-15 minutes, however, elapsed between the series of different rates, so that the effect of order was probably slight compared to what it might have been had the reactions at one rate immediately followed those at another rate. Aside from somewhat lesser

FIGURE 4

Graphic Representation of the Data of Table IV.

The intervals between the successive sounds of a series are represented as abscissae and the average reaction time in σ as ordinates. Br and Hh refer to the two subjects.



regularity, the curves of Fig. 4 agree in a general way with those of Figs. 2 and 3. Both sets of curves show the same marked increase in reaction time with prolongation beyond a certain most favorable interval or range of intervals; and beyond the point where this increase is first noticeable, in both cases, the increase is at first more rapid than it is later, with uniform increase in the duration of the intervals. While in obtaining the results of Table IV no particular warning signal was used, evidently in a regular series of sounds such as is here in question, each sound serves as a warning signal for the next, so that it may be said that the procedure for both Table I and Table IV involved the repetition, over and over again in succession, of the same preparatory interval; and it is this likeness in the two procedures that renders it justifiable to regard the data of Table IV as confirming the data of Table I, and at the same time explains the wide dissimilarity in the results obtained, on the one hand, by either of these procedures and, on the other hand, by the procedure in which the preparatory intervals are mixed irregularly. The prolongation in the reaction time for slower rates shown in Table IV, confirms Schumann's statement that the adaptation becomes much more difficult for rates much slower than one sound every 2 secs.[10a] That there is, however, a very considerable degree of adaptation for intervals considerably longer is shown by the fact that the reaction time does not reach its maximum until very slow rates are reached, with either the procedure of Table I or that of Table IV. The results presented in Table IV are not extended enough to fix the rate required before the reaction time reaches a maximum of slowness, but show that the rate is certainly not less than one sound every 8 secs., and probably well over one sound every 12 secs.; and the results of Table I indicate that the maximal reaction time is not reached until the interval is well over 24 secs. I am inclined to think, therefore, that Schumann's explanation of our estimation of short intervals applies equally well to longer intervals, for while it is true that perfect adaptation is not possible much beyond an interval of 2 secs., there is nevertheless a degree of adaptation present, and the degree of this adaptation does not decrease any faster than does the keenness of temporal estimation. It is true that there is less adaptation of attention in the case of a rate of one sound every 8 secs. than in the case of one sound every 2 secs., but the absolute accuracy with which we estimate 8 secs. is much less than that with which we estimate 2 secs. In brief, the degree of adaptation of attention runs parallel to the accuracy of temporal estimation.

[10a] For further data bearing on Schumann's theory, see the author's *A Quantitative Study of Rhythm, Archives of Psychol.*, 1909, **14**, 10, 37, 61-62.

In confirmation of this conclusion, I may mention that the feelings of surprise and strain which Schumann mentions as occurring in the comparison of intervals of less than 2 secs., are very marked in much longer intervals. After using preparatory intervals of 16 or 20 secs., an interval of 4 or 8 secs. seems short; and the stimulus coming at the end of this short interval usually produces a peculiar, but very noticeable, feeling of surprise, finds the subject unprepared, and results in a long reaction time. Whether the stimulus coming at the end of any interval produces surprise or is preceded by expectant strain, depends upon the place of that interval in the series of intervals that is being used. A further study of this effect of order of intervals is presented in Tables VI and VII.

Before leaving the question of the bearing of the results of Tables I and IV upon the perception of time, I wish to point out one further conclusion, one which offers an explanation of the law governing the effect of interval on reaction time but at the same time runs counter to a great many published but unproved statements with respect to the perception of time. The results of Table I show that when a regularly repeated preparatory interval is used, the duration of the reaction increases with the longer intervals, the reaction time always equalling a constant plus the product of another constant into the logarithm of the interval. I shall argue further on that this increase in reaction time with the unfavorable intervals indicates that at the time the reaction occurs at the end of these longer intervals, there is not such a perfect readiness to react. The evidence and authority in favor of this explanation is abundant. But, however true this explanation, we have still to explain why there is less adaptation of attention or less readiness. Neither fatigue nor the effects of temporal adaptation offer any adequate explanation. If the subject could estimate the 20 sec. interval with the same absolute accuracy as the 2 sec. interval, however weak, wandering or fluctuating his attention, there is no reason why he could not be just as attentive at the end of 20 secs. as at the end of 2 secs. He would not need to maintain good attention during the whole interval, but only to be ready at the end of it. Consequently, if the interval of 20 secs. was estimated with the same degree of accuracy as that of 2 secs., there can be little doubt but that equally good adjustment of attention could be brought into play at the end of the interval, that is, at the time of the occurrence of the stimulus to which the subject had to react. It is evident then, that while we may offer lack of perfect adaptation of attention, or unpreparedness, as the immediate explanation of the prolongation in reaction time, which, as Tables I and IV show, occurs with the longer intervals, it is further true that this lessened degree of preparedness would not exist if the subject could estimate the longer intervals with the same degree of accuracy as the shorter. Consequently, the prolongation in reaction time indicates the decrease in the accuracy of the estimation of the intervals. As long as the reaction time is not as long as with a set of totally irregular intervals, there is evidently a certain degree of ability to estimate the interval. On the other hand, any prolongation of the reaction time in the case of the longer intervals beyond that obtained with an interval of 2 secs. plainly indicates that the longer interval is not estimated as accurately as is the interval of 2 secs. Since the increase in reaction time is perfectly smooth, gradual and regular from 2 secs. up to 24 secs., it is plain that the accuracy of the estimation of intervals decreases gradually and regularly from 2 secs. up to 24 secs. There is no sense in saying that an interval of 4-5 secs., or of 8 secs. or of 12 secs. is the longest which can be grasped as a single temporal perception.[11] We can perceive intervals that, at any rate, are quite long. There is no break at 4 secs. or 8 or 12 secs. All that happens between 2 secs. and 24 secs. is that the absolute accuracy of estimation of intervals decreases as the interval increases. The results of Table I show that this decrease in the accuracy of estimation must be regular and smooth from 2 up to at least 24 secs. The increase in reaction time with longer intervals, which indicates the decrease in accuracy of estimation of intervals, is identical with the increase in the product of a constant and the logarithm of the interval. This indicates that the estimation of intervals is in accordance with Weber's law from 2 to at least 24 secs. This law is not proved by the data here presented, of course, since it is not established in accordance with what law the degree of readiness to react varies with variation in the accuracy of the estimation of the interval. But if Weber's law holds for very short intervals as is usually stated,[12] it must hold up to 24 secs., since the same law holds for this whole range of intervals, in the case of what is a function of the accuracy of the estimation of the interval, namely, the reaction time. What the data here presented do prove, therefore, is that the estimation of time follows some regular law which holds without break right through the various intervals so often mentioned as the longest that are "immediately" perceived.

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[11] "Im allgemeinen dürfte dann unter günstigen Umständen der Zeitwerth von 4-5 Sec. die obere Grenze bezeichnen, bis zu der eine Zeitvorstellung als

None of the subjects gave the introspection that they ever

Ganzes unmittelbar zusammen gefasst werden kann, wogegen diese Grenze auf höchstens 1.5 bis 2 Sec. herabsenkt, wenn die Zeitstrecke ungegliedert ist." Wundt, *Physiologische Psychologie,* 5th ed., 1903, vol. III, 49.

"As a matter of fact our *direct,* as distinguished from our indirect and inferred, consciousness of time, never exceeds a few seconds. Under favorable conditions it may mount up to 12 secs. or thereabouts, but ordinarily it it much shorter." Angell, *Psychology*, 4th ed., 1908, 191.

"The conscious present varies greatly in objective duration; but there can be no doubt that it may last for a considerable period of time: it is 'now' during the whole hour that we spend in the dentist's chair, or during the whole morning that we devote to some baffling problem." Titchener, *A Text-book of Psychology,* 1909, 341.

[12] "Innerhalb jener beiden Grenzen, dem Indifferenzwerth als unterer und dem Maximalumfang des Bewusstseins als oberer, folgt nun die Unterschiedsempfindlichkeit für Zeitgrössen ziemlich genau dem Weberschen Gesetze." Wundt, *Physiologische Psychologie,* 5th ed., 1903, vol. III, 49.

tried to estimate the interval by means of breathing. This very complicating factor was avoided, presumably, because the instructions were to keep the attention on the reaction. They were not primarily engaged in estimating intervals. Yet they tried to estimate the interval so as to be perfectly ready to react at the exact moment the stimulus was given.

I wish at this point to make a few comparisons between data so far presented and some results that have been obtained by others, whether by procedures similar to the one used for Table I, or by the procedure in which preparatory intervals of different lengths are mixed in irregular order. The results so far presented on the effect of prolongation of the preparatory interval offer considerable contrast and at the same time considerable agreement with results already published by others. The present data show a much more marked prolongation in reaction time as the result of lengthening the interval, and also greater uniformity or smoothness in the curve which represents this prolongation. My results agree with those of a number of investigators respecting the favorableness of the 2 sec. interval. These investigators[13] have all found that some quite short preparatory interval, about 2 secs., is the most favorable; that is, gives the shortest reaction times. Various sets of intervals have been used, but in most cases only a few different intervals, with a maximum interval of only a few secs.; yet there is very close agreement in the results. It may be concluded with a high degree of certainty, that a preparatory interval of from 1 to 4 secs. is the most favorable. There is strong evidence that, in many cases at least, the interval is extremely near 2 secs. Whether there is just one exact interval which may be said to be most favorable, or whether there is a little stretch of intervals lying between 1 and 4 secs. about equally favorable, is hard to determine. The most favorable interval apparently holds for all kinds of reaction times, motor or sensory, simple or discrimination, and for persons of widely differing ages, though the effect of age on the most favorable interval has never been particularly investigated.

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[13] Dwelshauvers, *op. cit.;* Moore, *op. cit.;* Della Valle, *op. cit.;* Breitwieser, *op. cit.*

There is less unanimity of opinion concerning what happens when the preparatory interval is increased beyond 2 secs. Della Valle used series of intervals varying by steps of 2 secs. from 2 to 8 secs. for one subject and from 2 to 14 secs. for another. The intervals were mixed in irregular order. He used seven subjects, but publishes, as curves only—no averages or mean variations—the results obtained with two subjects. The number of reactions with each interval was only 10 or 12. These curves show an extremely large and well marked "attention wave," of a period of 4 secs. Thus, in one case, the reaction time at 2 secs. is 122 σ, at 4 secs., 138 σ and at 6 secs., 115 σ. This regular undulation continues, together with a rise in the general height of the curve up to 14 secs. Breitwieser refers to the regularity and uniformity of these fluctuations found by Della Valle as "remarkable," and comes to a conclusion in "entire disagreement"[14] with Della Valle. My results are also in entire disagreement.

The data published by Breitwieser are more complete and reliable and it is worth while to consider his conclusions very carefully. He found, like previous investigators that "an interval of 2 or 3 secs. appears to be in general the most favorable."[15] He found, however, that the influence of interval is slight, when the average of all the subjects is considered, while in the case of the individual records there was a good deal of fluctuation. Speaking of the general average, Breitwieser writes: "The reaction time after one interval differs but little from the reaction time after another interval." In the case of sound the average of all reactions for a 2 sec. interval was 130 σ; for 9 secs., 136 σ; and for 10 secs., 134 σ. In the case of reactions to visual stimuli the average for the 4 sec. interval was 177 σ; for 2 secs., 186 σ; for 9 secs., 196 σ; for 10 secs., 195 σ. In these cases, *the subject never knew which interval to expect.* Ten readings were taken for each interval, but the intervals were given in a promiscuous order. No further indications of the order are given. We may, then, according to Breitwieser's results, sum up the effect of variation in the interval, when the intervals are used irregularly and without foreknowledge of their length on the part of the subject, by saying that in general the most favorable period is from 2-4 secs., but that there is only a very slight lengthening of reaction time as the interval is increased up to 10 secs., or as it is shortened to 1 sec. The change in reaction time with change in interval may, moreover, according to Breitwieser's results, be found to be rather irregular.

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[14] Breitwieser, *op. cit.,* 37.

[15] *Op. cit.,* 24.

In addition to these results Breitwieser used a procedure, in the case of one subject, where the subject knew in advance what length of interval was to occur between the ready signal and the stimulus. The ten reactions with each interval were taken in sequence before passing to another interval. He does not state however, in what order the different intervals were taken. "The results show that foreknowledge of the interval makes the reaction quicker, and much more uniform throughout the range of intervals." "Still, the interval of 1 sec. is less favorable than those of 2 and 3 secs.; and the longer intervals again give somewhat slower reactions. . . . It appears thus that the subject cannot readily adjust himself for the reaction within the first second after the ready signal, and also that he is not likely to be at his best at the end of an interval of over 4 secs."[16] The prolongation of reaction time found by Breitwieser in this case was from 117.8 σ (shortest) at 2 secs. to 132.6 σ (longest) at 9 secs.—much greater than with regular procedure.

We have, then, to distinguish two main types of procedure. First, the method of mixing the preparatory intervals in an irregular order, without foreknowledge on the part of the subject as to the length of the intervals; and second, the method wherein each interval is repeated a number of times before passing on to the next longer interval, with foreknowledge on the part of the subject. As Breitwieser used eighteen subjects with the first procedure and only one, apparently a different one from any of the other eighteen, with the second, it is hard to compare his results on the two procedures. I would like to emphasize, perhaps a little more than Breitwieser himself has done, the greater uniformity obtained by the method of regular repetition with foreknowledge, and also what his results do not show very well, the greater prolongation obtained by the regular procedure as the result of prolongation of the preparatory interval. This effect of method, together with the fact that I extended my intervals up to 24 secs., serves to harmonize my results very well with those of Breitwieser, and explains why I found a more marked and uniform effect as the result of the prolongation of interval.

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[16] *Op. cit.*, 30.

To make more sure of the effect of the order in which the intervals were given, and of foreknowledge on the part of the subject, I made some additional experiments with the same three subjects, whose results are plotted in the three curves of Fig. 2. As already explained, the procedure used in obtaining the data represented by the curves of Fig. 2 was to give the same interval 25 times in succession, the subject being told beforehand of the length of the interval and of the fact that it would not be changed until he was so informed, and then, after taking these 25 reactions with the same preparatory interval, to pass on to the next longer interval. The first interval used was always the shortest, i.e., 1 sec., and then the intervals were taken in order up to 24 or 28 secs. I now took a large number of reactions in which I mixed up in irregular order intervals varying in length from 4 to 20 secs. I kept track of the order but the subject never knew beforehand which of these intervals would be given. The order in which I gave the preparatory intervals was as follows: 4, 16, 20, 4, 8, 20, 12, 16, 8, 4, 12, 4, 20, 8, 12, 4, 16, 20, 8, 16, 12, 12, 8, 16, 20. This list of intervals is too long and too irregular for a subject to learn. I occasionally changed the order of the first few intervals, however, and further, it is likely that the subjects came to realize that every interval occurred about equally often, and that the same interval was but seldom given twice in succession. The introspections, however, of all three agree that it was never known just how long an interval to expect.

The results are represented in Table V. The average reaction time in σ and the mean variation of the average of all reactions obtained with each interval is given for each of three subjects, under the columns headed R.T. and M.V. The intervals are given in the first horizontal line of the table. The lowest line gives the average of the averages for the three subjects, equal weight being given to the final determinations in the case of each subject.

TABLE V

Effect of Interval on Reaction Time, Irregular Procedure.

N, for each average, for Sz, 125; for Vs, 100; for Ww, 50.
Total No. of reactions = 1,375.

Interval	4		8		12		16		20	
Subject	R.T.	M.V.	R.T.	M.V.	R.T.	M.V.	R.T.	M.V.	R.T.	M.V.
Sz	211	*34*	207	*25*	216	*22*	218	*25*	232	*29*
Vs	204	*34*	199	*30*	202	*33*	204	*37*	213	*39*
Ww	214	*35*	199	*33*	178	*24*	178	*23*	170	*32*
Average	**210**		**202**		**199**		**200**		**205**	

The results included in Table V may be best discussed in comparison with those of Table I, in which was used the method of varying the preparatory interval in a regular and known order, from shortest to longest, repeating each interval a number of times as already described. To facilitate this comparison the average result for each interval with each procedure is given, and the general tendencies represented by these averages are plotted as curves in Fig. 5.

Comparison of the Effect of Regular and Irregular Variation of Preparatory Interval on Reaction Time.

Interval in secs.	2	4	8	12	16	20
R. T., Regular Procedure	136	150	168	181	189	197
R. T., Irregular Procedure		210	202	199	200	205

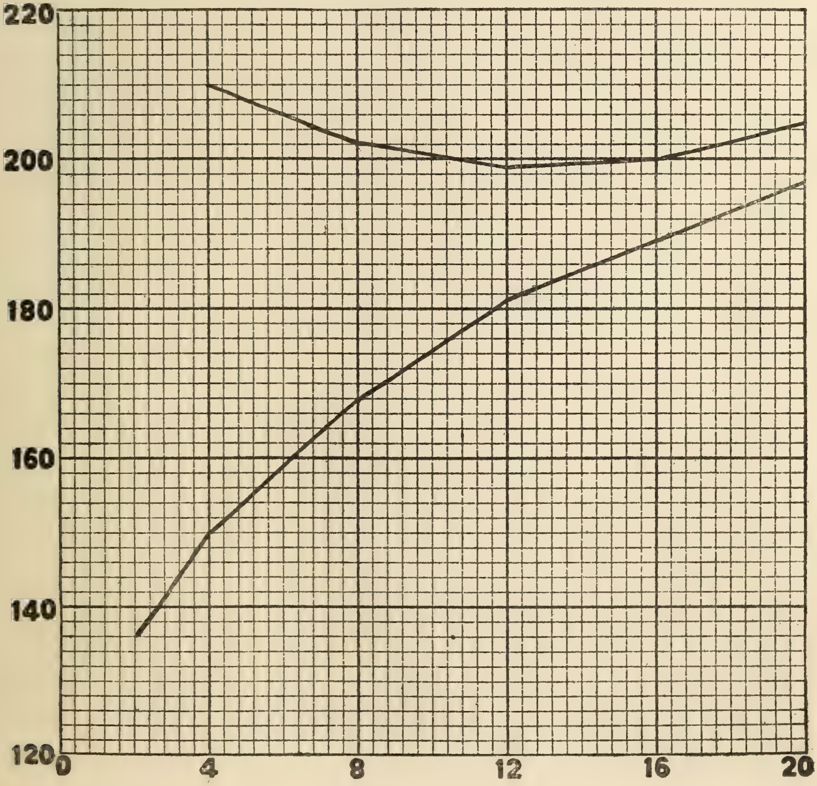
There are some interesting differences in the results obtained by the two procedures. In the regular procedure the reaction time increases, rather uniformly, at first faster than later, as the duration of the preparatory interval is increased beyond 2 secs. With the irregular procedure, the effect of variation in the preparatory intervals is very much less marked. In the regular procedure the difference in time between the 2 sec. interval reaction time and the 20 sec. interval reaction time is 61 σ, whereas with the irregular procedure the maximal difference (between the reaction times with a 12 sec. interval and that with a 4 sec. interval) is only 11 σ.

Yet a rather definite effect of the variation in interval is evident even in the case of the irregular procedure. The shortest

FIGURE 5

Graphic Representation of the Effect of Regular and Irregular Variation of Preparatory Interval on Reaction Time.

The durations of the preparatory intervals in secs. are represented by the abscissae and the rection time in σ by the ordinates. The upper curve represents the results obtained with the irregular procedure, and the lower one, the results obtained with the regular procedure.



reactions are obtained not with the intervals nearest 2 secs., but with an interval towards the middle of the series used, in the above case 12 secs. Subjects Sz and Vs give the shortest reactions with intervals somewhat shorter, and subject Ww with intervals somewhat longer, than the middle interval. The intervals lying on either side of that giving the shortest reaction time, in general, tend to give longer reaction times, but this increase in length is more noticeable in the direction of the shortest intervals, and indeed not at all noticeable in the case of subject Ww in the case of the longest intervals. I have obtained similar results even when the shortest interval used was 3 secs., a very favorable interval with the regular procedure. When mixed irregularly with a series of longer intervals, the 3 sec. interval gave a longer reaction time than any of the others, so that while it is a very favorable interval with the regular procedure, it may be a most unfavorable with the irregular. A great deal depends upon the order in which the intervals are mixed, but often the most striking effect of interval when the irregular order is used is to make the reactions with the shortest interval the longest. From a variety of experiments I have made (some of which are presented below), I believe this lengthening of reaction time with the shortest intervals will hold for any series of fairly short intervals, whether the shortest interval be 1 sec., 2 secs., 4 secs., or longer. Another striking point is that the irregular procedure gives much longer reaction times. The difference is especially marked in the case of the intervals down towards 2 secs., and becomes less so as the interval increases in length. The difference in time for the same interval, in the case of the two procedures, grows less and less as the interval increases in length, as the result of the increase in the length of the reaction time with the regular procedure, but it still holds up to 20 secs. that the regular procedure gives the shorter times. The course of the two curves in Fig. 5 indicates that with long enough intervals the regular procedure would give just as long reaction times as the irregular; that is, that the curves would at some point coincide. At what point this would occur is hard to estimate, but it is rather surprising to find it well above 20 secs. As near as I can estimate, the curves would coincide at an interval somewhere about 32 secs. It has to be taken into account that introducing longer intervals into the irregular series would probably raise the general level of the reaction times obtained from the series. This makes it rather difficult to give a general answer to the question at what duration of interval the regular procedure would give as long times as the irregular. It depends somewhat on the length of the irregular intervals.

Before attempting to explain the differences in the results obtained with the regular and the irregular procedures, I shall present some data showing how important the order of the intervals is, when they are mixed irregularly. Using the irregular procedure, it seemed to me that I could obtain the longest time with any interval I chose, by arranging the intervals in the proper order. That this can always be done is improbable, even when the subject's individual tendencies are known, but it is nevertheless certain that the order makes a very great difference. In Tables VI and VII are given the results obtained on several subjects together with the procedure used. The intervals are given in the top line of the table in secs. in regular order. In Table VII the irregular order actually used is given. Only ten reactions were taken with each interval in each procedure, so these results are not very reliable. The mean variation is omitted; it is of no particular importance here and would probably add confusing figures.

Table VI

Effect of Order of Intervals: Results Arranged in Regular Order, Obtained by Both the Regular and the Irregular Procedure.

Subject	Procedure	3	5	7	10	13	16	19
W	Regular	99	102	122	123	137	145	135
"	"	86	94	117	127	129	140	151
" (Series A)	Irregular	166	146	165	140	152	125	143
K	Regular	125	126	144	142	162	158	164
" (Series B)	Irregular	226	194	185	163	165	216	191

Interval	....	3	3.5	4	4.5	5	5.5	6	6.5	7
M	Regular	113	129	136	138	138	142	147	143	157
" (Series C)	Irregular	138	145	146			139		140	144
" (Series D)	"	220	189	168	174	168	159	162	174	158
C (Series E)	"	226	218	184	205	206	194	195	209	195

Table VII

Results with Irregular Procedure, with Intervals Arranged in the Order Used.

Order of Intervals	5	10	19	7	3	13	16		
Subject W (Series A)	146	140	143	165	166	152	125		
Order of Intervals	13	19	5	10	16	3	7		
Subject K (Series B)	165	191	194	163	216	226	185		
Order of Intervals	6.5	4	3.5	3	7	5.5			
Subject M (Series C)	140	146	145	138	144	139			
Order of Intervals	6.5	3	5.5	3.5	4	5	7	6	4.5
Subject M (Series D)	174	220	159	189	168	168	158	162	174
Subject C (Series E)	209	226	194	218	184	206	195	195	205

Table VI merely confirms the results already presented. Where the regular procedure occurs, it shows the regular increase in time with increase in the duration of the preparatory interval. Where the irregular procedure is used, it shows the greater absolute length of the general run of the reaction times, and shows the longest reaction times with the shortest interval. Only in one irregular series, series C, subject M, is the shortest reaction time obtained with the shortest interval, and the order of intervals used in this series was planned with the object of placing the shortest interval in the most favorable position for it in the series, namely, immediately after the intervals which had most nearly the same length.

Table VII shows in the case of four subjects just what the effect of order of intervals was, as the intervals are arranged in the table, from left to right in the order they were given. It is rather hard to explain each individual time in the table. A good deal must be allowed for accidental variation. Also, the general rule must be borne in mind, that, with the irregular procedure, the middle intervals give the shortest times, while the longer intervals give slightly longer, and the shortest intervals the longest times. This has already been shown in Table V. It is well shown by subject K, series B, in Table VI. The order of the intervals, however, is a complicating factor. In the case of the subjects of Table VII, the most marked effect of order seems to be that the reaction time to any interval is shortened when preceded by an interval of nearly the same or shorter length, and that it is lengthened when preceded by a considerably longer interval. Table VII bears this out, though it is probably hardly worth while to consider each individual reaction time. The explanation would probably be that there is adaptation on the part of the subject not only to the general trend of the series, but that the last interval has a little more effect upon his attitude than less recent intervals.

Let us suppose, for example, that the subject does not get quite ready in time for the stimulus in the case of the shortest preparatory intervals, because of a tendency to adapt to the intervals of middle or slightly shorter length, and further that the interval to which his attention tends to adapt itself is determined largely by the last preceding interval. If the last preceding interval were a long one, he would tend not to be ready so early as if this preceding interval were a short one. This would make his reaction to a following short interval somewhat longer because of lack of adaptation, or unreadiness. On the other hand, if the preceding interval had been one of the same length, the stimulus at the end of the next interval would be more apt to find the subject just ready. Or if the preceding interval had been shorter, the subject would be ready too soon, but would attempt to maintain his adaptation, perhaps increase it, so that if the next interval were not *too* long, the stimulus at the end of it would probably find the subject quite ready, and give a comparatively short reaction time for that interval. In other words, the results of Table VII on the effect of order are readily understood if we assume that the subject gets perfectly ready to react in time only for stimuli which follow intervals as long or nearly as long as the middle interval of the series: that this is an energy-saving habit which he more or less unintentionally acquires as he becomes familiar with the general run of the intervals; but that this habit is slightly modified by each interval that comes along. This modification is of such a sort that each time he tends to get ready earlier or later than usual according to whether the last preceding interval was shorter or longer than the middle intervals. This amounts to saying merely that the subject's most recent reaction experiences have somewhat greater weight in determining his present behavior, in the way of getting ready, than the more remote ones of the series, though all contribute.

Now I would like to be understood as expressly denying that the effect of order of intervals must necessarily be such as I have claimed it to be with the subjects of Tables VI and VII. That my surmises concerning these subjects are correct is borne out by the fact that experimenting with them I was able to arrange series so as to have the interval of 3 secs. give the shortest reaction time—instead of what is easier, to have it give the longest—this, with series of intervals in which 3 secs. was the shortest interval. An instance of this is found in Table VI, series C and D, subject M. But it is highly probable that other subjects would behave differently. Getting an idea of the nature of the order, they might learn to expect a shorter interval after a long, and not to expect two short ones in succession, etc. Some subjects might be always well prepared for the longest intervals of the series, since after they once knew the limits of the series, if the stimulus had not come after a considerable wait, they could be pretty sure that it was just about due. The effect of order is, no doubt, very complicated, and some subjects would quite certainly behave very capriciously as regards it. At the same time it is highly probable that the process of adaptation as I have described it is very common and one of the chief factors determining the effect of the order of the intervals upon reaction time. This seems likely as the same phenomenon is widely found in the estimation of time. After leaving a long interval, especially one we have had to listen through attentively, a short interval seems unusually short, and vice versa. After working as subject with preparatory intervals of 16 and 20 secs., and then coming back to intervals of 4 secs., I have found them apparently so short that I have gotten up and gone out to see whether the experimenter had not made a mistake and used a 2 sec. interval instead of the 4 sec. one that I desired. The bearing that these results on the effect of interval on reaction time have upon the psychology of time perception, and their similarity to those obtained by Schumann with very short intervals, I have already pointed out. Following the general lines of Schumann's theory, the above-mentioned underestimation of the 4 sec. interval would be due to the fact that the stimulus at its end comes before one is ready for it, and there is a feeling of surprise. The longer interval is overestimated because we get ready too soon for the sound terminating it, and having gotten ready, we have to try to maintain our readiness until it does come. There thus results a feeling or a sensation of strain.[17]

I now feel that I have presented fairly fully the facts of the effect of variation in interval on reaction time. I desire next to take up the explanation of these facts, and especially the explanation of the prolongation in reaction time which occurs as the preparatory interval is changed from 2 secs. to longer intervals, or when a number of preparatory intervals of wide range in duration are used in irregular order. I shall argue that this prolongation is due to the fact that, under the circumstances under which it occurs, the attention of the subject is not maximally adjusted or adapted for the task in hand. This argument is fully justified by the existing literature, though my own observations are sufficient to convince me of its correctness.

As already stated, previous investigators have come to the conclusion that a preparatory interval of 2 secs. gives the shortest times. What explanation do they offer? It will be seen that there is quite uniform opinion that it is a matter of attention. The underlying thought, throughout, is that it takes about 2 secs. for the individual to get as ready as he can get, to adapt or adjust his attention or to obtain a maximum of attention, and that after 2 secs. the degree of attention decreases, either regularly or in a wave-like manner. In the cases where no warning signal at all or irregularly mixed preparatory intervals are used, the maximum adaptation of attention will only seldom coincide with the occurrence of the stimulus.

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[17] Wundt writes as follows: "Unerwartete Eindrücke können nun, auch wenn sie von mässiger, ja sehr geringer Stärke sind, eine dem Schreck verwandte Wirkung hervorbringen." *Physiologische Psychologie,* 5th ed., 1903, vol. III, 433. I include any inhibition effect that surprise may have as part of the story when I say that the reaction is longer because the subject is not ready. It is only because of the lack of a suitable preparatory adaptation of attention that the surprise occurs.

Wundt, in discussing the commonly obtained result that a preparatory interval of from 1 to 2 secs. gives the shortest times, writes: "Ist die Zeit kürzer, so kann eine hinreichende Spannung der Aufmerksamkeit nicht mehr eintreten, ist sie länger, so machen sich die oben bemerkten Schwankungen geltend."[18] The undulations of attention referred to are those constituting the classical "attention wave." The conception is that after maximal strain of attention is once secured, if the subject has longer to wait, the attention begins to oscillate, and at the same time decreases somewhat in general level.

Pillsbury writes as follows: "It is found that unless the attention is aroused by a suitable signal, given about two seconds before the stimulus that is to call out the reaction, that the movement does not reach its maximum quickness. The signal gives the highest degree of adaptation of the attention, and so is but a sub-head under the preceding" [the effect of attention upon the rapidity of movements]. "It is also significant that the period by which the signal must precede the stimulus is nearly half of the length of an attention wave, so that part of the adaptation may consist in the time it takes for the attention to rise to its highest point."[19]

Ladd and Woodworth in their chapter on the temporal relations of mental phenomena offer only the following explanation of the effect of the preparatory interval, namely, that a shorter preparatory interval than 2 secs. does not allow the subject to prepare himself fully for the stimulus, "while a longer period than 2 secs. allows more time than is needed and so affords a chance for wandering of the attention."[20]

Dwelshauvers entitles his study of reaction time "Untersuchungen zur Mechanik der activen Aufmerksamkeit."[21] He found that an interval of 1.5 secs. gave shorter reactions, especially in the case of sensory reactions, though in the motor as well, than did an interval of 3 or 6 secs., the 6 sec. interval giving the longest. He also found that longer reaction times occurred without any signal than with a signal. The following quotations contain his explanation. "Die Reagenten erklärten, dass 1½ und 3 Sec.-Intervalle sich als sehr bequem erwiesen zur gehörigen Spannung der Aufmerksamkeit; 6 Sec.-Intervalle dagegen kamen ihnen unangenehm und ermüdend vor."[22] "Das Signal begünstigt im allgemeinen die Anpassung der Aufmerksamkeit. Die Reactionen, welchen ein Signal vorangeht, sind daher durchweg kürzer."[23]

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[18] Wundt, *Physiologische Psychologie,* 5th ed., 1903, vol. III, 434.

[19] Pillsbury, *Attention,* 1908, 82.

[20] Ladd and Woodworth, *Elements of Physiological Psychology,* 1911, 482.

[21] *Phil. Stud.,* 1891, 217-249.

Bliss, in speaking of the effect of a warning signal writes that the effect of the signal on the reaction time depends on the interval between the signal and the stimulus. "If the interval is too short there is not time enough to concentrate the attention and the warning hinders the reaction instead of helping it. If the time is too long the effect dies away, as the mind is unable to keep its maximum attention for more than 1 or 2 secs."[24]

Moore writes: "The study was commenced with those factors which have been supposed to affect reaction time by their influence on the attention."[25] Among these factors that affect reaction time by influencing attention, the first that he mentions is the duration of the interval between the preparatory signal and the stimulus. His results indicate that when the preparatory signal comes 2 secs. before the reaction stimulus, the conditions are slightly more favorable for reaction time than when it comes at either 1 or 3 secs. before. Moore also studied the effect of an irregular preparatory signal, and found it to consist in a very marked prolongation of the reaction time. In way of explanation, he writes as follows: "The difficulty under which the

[22] *Op. cit.*, 226.

[23] *Op. cit.*, 229.

[24] *Investigations in Reaction-Time and Attention, Yale Psychological Laboratory Studies,* I, 1893, 16-17.

[25] *A Study of Reaction Time and Movement, Psych. Rev. Monog. Suppl.*, 6, 1904-1905, 31.

attention labors in preparing for a reaction when the preparatory signal comes at 1 or 3 secs. beforehand, is certainly slight if compared with the strain of keeping in readiness for reaction when the warning signal is given at irregular intervals or not at all. At the end of such a series of experiments there is a feeling of relief which is indicative of the strain under which the attention was laboring. Previous investigators have found that the efforts of a subject to keep his attention focused were not successful and that the time of reaction was considerably lengthened."[26]

Vaschide and Meunier[27] are very explicit in interpreting the prolongation in reaction time produced by irregular intervals or the omission of a preparatory signal as due to the lack of accommodation of attention. In discussing Wundt's results which show that shorter reaction times are obtained with a preparatory signal than without such, they write: "La raccourcissement du temps de réaction ne peut s'expliquer dans ces cas que par une *tension préparatoire de l'attention.*"[28]

"L'indétermination de l'excitation, soit quant au temps, soit quant à l'intensité, est aussi a considérer. . . ."[29]

"Toutes ces prolongations du temps de réaction semblent avoir origine dans une prolongation de la *durée de l'aperception,* rien ne prouvant véritablement une modification des temps psychophysiques et physiologiques."[30]

These authors seem so convinced that the prolongation in reaction time resulting from the absence of a signal is due to the effect upon attention, that they reproduce the proposals made by Vaschide in his joint publication with Toulouse et Piéron, *Technique de Psychologie expérimentale,* concerning the experimental procedure to be followed when one uses reaction times to obtain an appreciation of the subject's power of attention.[31] This procedure resembles that which I have used in the present work, and in brief is as follows: First, 25 simple tactile reactions, with a 2 sec. preparatory interval. Second, 25 simple tactile reactions, without preparatory signal, the stimuli following each other at intervals of irregularly varying length. As the intervals between the stimuli, it is proposed to use the following ten in the order given, the intervals being given in secs.,—5, 10, 10, 5, 10, 15, 5, 5, 10, 5. The maximum and the minimum reaction times of each series is to be noted, but the relative mean variations, that is, the ratios of the mean variations to the averages is regarded as a more precise measure of attention. The difference between the average of the reactions with a 2 sec. interval and of those without a preparatory signal is also suggested as a measure of attention. Choice reactions are also proposed, the same procedure being followed with them as with the simple tactile reactions.

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[26] *Op. cit.,* 33.

[27] *La Psychologie de l'Attention,* 1910.

[28] *Op. cit.,* 21.

[29] *Ibid.,* 20.

[30] *Ibid.,* 21.

[31] . . . "lors qu'on emploie la mesure du temps de réaction à l'appréciation psychologique de l'attention."—*Ibid.,* 44.

Breitwieser writes as follows: "The most favorable interval must lie between intervals that are less favorable. *A priori,* the concept would seem to be justified beyond question; for, if a subject is not already fully prepared to react on receiving the ready signal, some time must be required for him to reach his maximal degree of preparedness; and again, this maximal degree of preparedness can scarcely be maintained indefinitely." He writes that the subjects often said they found it hard to hold the attention steadily for the longer intervals. That Breitwieser regards the degree of attention as the source of variation in reaction time with variations in interval is shown further by the fact that he uses his results to conclude against an attention wave, concerning which he writes as follows: "Now it would seem that the reaction time ought to be a fairly delicate indicator of flucutations of attention, or of readiness to react; if it indicates so clearly the existence of a most favorable interval, it ought also to indicate the existence of any physiological rhythm of a regular and universal character, exercising an influence upon cerebral efficiency. Our results seem to justify a conclusion adverse to such a rhythm, so far as it could be expected to appear within a space of 10 secs." ". . . our experiments have discovered one type of wave which does remain constant; but this is a very simple type, consisting simply in the occurrence of a most favorable interval."[32]

The above citations are sufficient to show what is usually regarded as the explanation of the prolongation in reaction time which results when long preparatory intervals or none at all are used in place of one of about 2 secs. If the conclusions of the authors I have cited are correct, we must evidently regard it as established that one cannot maintain a maximum adaptation of attention for more than a few seconds, if indeed that long, and further that any letting up of this adaptation will show itself in the reaction experiment by longer times. I think it may be said that there is unanimity of opinion that the prolongation produced in reaction time by unfavorable intervals is due to a lesser degree of adaptation or adjustment of attention. This unanimity of opinions seems to be well justified, not only from theoretical considerations, but on the basis of the experimental evidence. The experimental evidence consists in the introspections of the subjects who have participated in experiments on the effect of variation in interval. Whenever reported, these introspections have been to the effect that with long preparatory intervals or without a preparatory signal, it was impossible to know just when to get most ready, to know just when to expect the stimulus, just how near it was to the time to react, that it was a great strain trying to pay attention through a long interval and that with long or irregular intervals the maximum readiness to react was seldom and only accidentally secured. The maximum degree of attention may at times exist for particular reactions, but more often does not and so, on the average, longer times are obtained.

At this point it is, perhaps, necessary to raise the question whether readiness to react really means adaptation of attention. I believe that this is in agreement with prevalent terminology. The greater the attention to anything, the greater its clearness.

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[32] *Op. cit.,* 35-38.

But adaptation of attention is a process implying certain adjustment or accommodation of the nervous system, and secondarily of muscles. With the greater clearness there must go a change in the nervous system. And in expectant attention preparatory to a reaction, there must be an adaptation of the nervous system for the execution of the reaction rather than for the performance of some other action. In short, while increase in clearness is a striking result of attention, increased efficiency is also a result. Everyone admits, it seems to me, that increasing the degree of attention shortens reaction time, and the reaction being a process involving the nervous system, this would not happen unless with higher degrees of attention there occurred an accommodation of some sort of the parts of the nervous system involved both in attention and in the reaction. By adaptation of attention, I mean this nervous process together with the mental parallel. I conclude, then, that it is an established fact that reaction time varies with the degree of adaptation of attention that exists at the time of the occurrence of the stimulus, and that the effect on reaction time of variation in the preparatory interval is due solely to the variation in the degree of adaptation of attention.

With respect to the prolongation in reaction time with the regular procedure, shown in Table I, where each day's work began with the shortest interval and ended with the longest, it might be objected that the subjects reacted more slowly in the case of the longer intervals simply because they had become fatigued by the previous reactions. That this was not the case is certain from results I have obtained with other subjects by the same procedure in which a similar prolongation in reaction time with prolongation in the preparatory interval was found. But in these subjects, after finishing with the 20 sec. interval, without pause except to notify the subject of the change in interval, I proceeded to a regularly repeated 2 sec. interval. It was found that these reactions to the regularly repeated 2 sec. interval, at the end of the hour's work, and after many reactions with the long intervals, gave a shorter average reaction time than the reactions with the 2 sec. interval at the beginning of the hour.

As regards the irregular procedure, any idea that the reactions with the irregularly mixed intervals were longer than those with a regularly repeated 2 sec. interval on account of a state of fatigue upon the part of the subject should be dispelled by the results presented in Tables XVII and XVIII, Chapter V. These tables present reaction times with the regularly repeated 2 sec. interval taken both before and after reactions to a set of irregularly mixed intervals. While the irregular intervals give very much longer reaction times than the regularly repeated 2 sec. intervals, there is very little difference between the reaction times with the 2 sec. intervals before and after work with the irregularly mixed intervals.

In the preceding pages a good deal has been said about the effect of adaptation to one set of intervals upon the reaction time obtained with other intervals. The question may arise whether the prolongation in reaction time with the longer intervals may not be due merely to the previous adaptation to the shorter intervals. This seems improbable from certain theoretical considerations and impossible on the basis of an experimental test reported below. The study of the effect of order already presented shows that the reaction time to any interval is not lengthened, but if anything shortened, when it is preceded by an interval of nearly the same length or slightly shorter. And in the regular procedure all intervals are preceded by intervals of the same length or else by intervals only slightly shorter, as the change from the short intervals to the longer is made by small steps. It is only when an interval is preceded by longer ones that a marked prolongation is apt to occur in the reaction time obtained with it. This is the reason why, in using the regular procedure, I did not proceed in inverse order from the longest intervals back to the shortest. Such a procedure with some subjects results in a prolongation in reaction time obtained with the shorter intervals. In other subjects it does not. In general, the effect of order is greatly reduced when the subject has foreknowledge of the length of all preparatory intervals.

In order to determine experimentally whether the prolongation in reaction time with increase in the duration of the interval could possibly be due to the effect of adaptation to the shorter interval, the following experiment was made. With subject Sh, a practiced subject, the regular procedure, as it has already been described, was used on the first and third days. Intervals up to 20 secs. were used. On the second and fourth days, only reactions with the 20 sec. interval were taken. With the regular procedure the reaction times for the 2 sec. interval averaged 128 σ and for the 20 sec. intervals 178 σ. On the days when only 20 sec. intervals were used, the average reaction time for the 20 sec. interval was 173 σ, and for successive series of 25 reactions the averages were 180 σ, 186 σ, 163 σ and 170 σ. The reaction time with the 20 sec. intervals was then, on the average practically as long on those days when it was not preceded by shorter intervals as on those when it was. This result proves clearly that unless we assume the effect of adaptation to the short interval used on one day to hold over to the following day, the lengthening in reaction time with the longer intervals in the case of the regular procedure could not have been due to any effect left upon the subject by the preceding shorter intervals. We have here, also, further proof that the lengthening in reaction time with the longer intervals in the case of the regular procedure is not produced by fatigue resulting from the preceding shorter intervals, as the reaction time with the 20 sec. interval is just as long when not preceded by any shorter intervals.

Some authors have spoken of the inhibitory effect of feelings of surprise or of strain when the stimulus comes either before or after it is expected, and such undoubtedly occur, as I have myself already pointed out. But the phenomenon evidently depends upon adaptation of attention. Surprise occurs if the stimulus comes before maximal adaptation of attention and a feeling of strain if it does not come until after this maximum. It would be incorrect, therefore, to say that these feelings cause the lengthening of reaction time with long or irregular preparatory intervals. Both the time of reaction and the feelings of surprise or strain depend upon the fact that the subject did not expect the stimulus at the right time. Moreover, in a routine reaction experiment, where the subject is fully instructed beforehand, and sounds of moderate intensity are used, the "surprise" is a very mild experience. So far as my own experience is concerned it seems very much the same thing to me, in this case, to say that the sound surprised me and to say that it came before I was expecting it. The feeling of strain, in the case of the longer intervals, may at times be more definite; but this merely indicates the difficulty of maintaining a high degree of adaptation of attention for any length of time. I sometimes feel, especially when I am out of practice, a high degree of strain right at the time of reaction, with a 2 sec. interval, and yet give shorter reactions than with long intervals when no great strain was experienced. Strain would seem if anything to be correlated with high degree of accommodation of attention, but at any rate could certainly not explain the lengthening in reaction time with the long or irregular intervals.

There is, however, one other factor than accommodation of attention, as I have already mentioned, which plays a very important rôle in the effect of variation in interval, wherever the regular procedure is used. This is the perception of time.[33] With a regularly repeated interval the subject soon learns when to expect the stimulus, provided he can learn the interval. It it not necessary to raise the question as to what intervals may be estimated with the greatest accuracy relative to their length. But it is evident that short intervals of about 2 secs. may be estimated with much smaller absolute error than long intervals. It is quite certain that from about 2 secs. up, through the range of intervals used in this research, the absolute size of the error would more or less regularly increase, whether according to Weber's law or not. Now we do not know just how long it is possible to maintain a maximum degree of adaptation of attention. It takes about 2 secs. to reach this maximum, and it probably is not maintained for more than 1 sec.[34] Now some people are, of course, able to estimate an interval of 2 secs. more accurately than others, but it is rather doubtful if any normal individual would estimate this interval so inaccurately that the stimulus, coming at the end of a regularly repeated 2 sec. interval, would not find the subject at his maximum of adjustment. In other words, I do not believe any subject would err in his estimation of an interval of 2 secs. by an amount greater than the time that it would be possible for him to maintain an approximately maximal adjustment of attention. We have not yet data by which to answer this question with certainty, but I think for all practical purposes at least, we may assume that with a regularly repeated 2 sec. interval any normal individual, and probably also almost any abnormal individual, would be able to adjust himself to the reaction to as great a degree as is possible for him at all. With longer intervals, at any rate with intervals over 4 secs., since above this length of interval the reaction time is quite certainly longer, the case is different. Subjects would not be able to judge these intervals accurately enough to be always sure of reacting with a maximal degree of attention. The more inaccurately they estimated the interval, that is, the less accurately they could tell just when the stimulus was due, the less perfect would be the adaptation of attention, on the average, at the time of the reaction. As the absolute inaccuracy of their estimations of the intervals would increase with the length of the intervals, it is evident that the longer the interval, the lower would be the degree of attention, on the average, at the time of the reaction. This would explain the rather gradual and regular prolongation in reaction time with increase in length of interval, when the regular procedure is used.

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[33] See G. Duechler, "Beiträge zur Erforschung der Reaktionsformen," II Abhandlung, *Psychol. Stud.*, 1913, 128--129.

[34] "If the stimulus be absolutely simple and one is careful to record each

Now individuals would differ in their ability to estimate these intervals, and thus with the longer intervals differences in the ability to estimate intervals might play an important part in explaining differences in the amount of prolongation of reaction time with prolongation of interval. Since the duration of maxi-

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appearance of something else, it seems that one can hold attention strictly to a single thing for less than a second." Pillsbury, *The Essentials of Psychology*, 1911, 123.

mal adaptation of attention under given reaction conditions would be very nearly a constant no matter what the duration of the preparatory interval, and since the errors in estimating intervals on the other hand would increase in absolute size with their length, inaccuracy in estimating the intervals would become more detrimental to the likelihood of securing maximal adaptation of attention the greater the length of the interval. And the less the likelihood of maximal adaptation of attention, the less would be the degree of attention on the average, actually met with by the stimulus. So that while I believe that with an interval as short as 2 secs., estimation of time would not be a factor of any importance in causing individual differences, since with it all subjects would secure maximal adjustment, it would quite certainly have influence in the case of the longer intervals.

Theoretically speaking, it would be hard to say that there would be any interval so long that a further increase in its length would not result in still longer times. The longer the interval the greater would be both the mean variation and the absolute temporal range of the distribution of a number of estimates of its length, and the greater the range of distribution of these estimates, the smaller the proportion which would fall within the limits of that duration for which a maximal adaptation can be maintained, and the more frequently would the lesser degrees of attention occur at the moment of reaction. But as already shown, the reaction time increases only slowly after very long intervals are reached. This is probably because the very long intervals are estimated so poorly that very little adaptation of attention is possible anyway,—the subject realizes that it is hardly worth while to attempt to hold himself in special preparedness at any particular instant, and consequently adaptation of attention becomes practically insignificant in amount. So that all the extremely long intervals would probably give about the same reaction time because no particular adjustment would be possible, and a few seconds more or less would make but little difference. We can thus understand why the curve showing the prolongation in reaction time with increase in the duration of the preparatory interval rises at first faster and then slower, with regular increase in the duration of the preparatory interval. Adjustment is considerable at first and interfered with to a considerable degree by slight increases in the absolute variability of the estimations of the interval. But with longer intervals there is less adaptation anyway, and there is consequently less detrimental effect from further increases in the absolute variability of the estimations of the intervals.

It is thus evident that time estimation has a great deal to do with the prolongation in reaction with prolongation in preparatory interval in the case of the regular procedure. In the case of the irregular procedure, on the other hand, where the different intervals are mixed irregularly, the perception of time is of negligible importance, for in this case, there is no interval to be estimated. With the irregular procedure, there is no great degree of adjustment to any interval, as is shown by the comparative equality of the times for the different intervals, and by the fact, already mentioned, that the general run of the reaction times is much longer with the irregular than with the regular procedure. There is quite likely to arise, however, a tendency on the part of the subject to be slightly more prepared, on the average, for the intervals of about the middle length of the series used, and so to be slightly less prepared for the stimuli following the shortest intervals. The effect of the order of stimuli I have already discussed.

I believe now that I have offered the correct general explanation of the effects of variation of interval, and the differences in the effect of the two procedures, which I have referred to as the regular and irregular. I have only to mention the increase in reaction times with intervals shorter than 2 secs. I am not particularly interested in this point, but the generally accepted explanation is, of course, that the process of accommodation or adaptation of attention requires about 2 secs. to reach its maximum degree.

I wish lastly to refer to the use of long or unfavorable intervals as detractors.[35] If we regard a regularly repeated 2 sec. interval as the most favorable, regularly repeated long intervals or irregularly distributed intervals would be unfavorable, and the effect of the latter, which is to produce a lengthening of the reaction time, may be regarded as a detraction effect. If the explanation that I have given of their effect is correct, it is evident that in the case of the irregular procedure, where perception of time does not enter as a factor, we are dealing with a very pure form of detraction from attention. That is, the prolongation in reaction time is due to the fact that we place the subject under conditions which are not the most favorable for a high degree of adaptation of attention to the reaction. In the case of long intervals with regular procedure detraction is involved, but dependent upon the ability to estimate intervals. The quickness of the reaction is varied by operating upon the subject's estimate of intervals as well as upon the degree of his attention. With the irregular procedure, however, the prolongation is due entirely to the fact that there is less adaptation of attention. Consequently, if we wish to use unfavorable intervals as a detractor, evidently we must use the irregular procedure, except in the case where no comparisons are to be made between different individuals.

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[35] For definition of this term, see introductory chapter, p. 9.

To sum up, a regularly repeated 2 sec. interval gives the maximal adaptation or adjustment of attention of which the individual is capable. A given set of totally irregular intervals varying from 4 to 20 secs. constitutes a definite obstacle to the attainment of this maximal adaptation. Such a set of intervals produces prolongation of reaction time solely by its effect as a detractor from attention. We have here, then, a satisfactory method of detraction.

This method of detraction has advantages which make it far superior to any of the distractors of attention hitherto used as detractors.[36] It is well known that no satisfactory *dis*tractor has yet been found.[37] In any case where there is divided attention it is impossible to determine what proportion of attention is involved in each of the different processes. As regards other detractors, such as drugs, fatigue, etc., we have none so far as I know that can be shown to act exclusively on the process of attention, nor could we rely on such to constitute always the same amount of detracting influence. We get in many cases, undoubtedly, detraction from attention, perhaps principally that, but other detrimental effects as well, so we cannot say how much of the decrease in efficiency of the tested function is due to decrease in degree of attention and how much to an effect upon other factors than attention. Similarly with weakening the intensity of the stimulus. Intensity is undoubtedly a condition of attention, and in decreasing the intensity of the stimulus we decrease the degree of attention, other things equal, involved in its perception. But this lowered intensity of stimulus may cause prolongation of reaction time for other reasons than that of consequent lowered attention, as, for example, a longer latent period in the stimulation of the sense organ.[38] While unfavorable preparatory intervals may be the best detractor hitherto employed for the purpose of measurement of attention, nothing that has been said should be interpreted as meaning that other equally good detractors may not be discovered. The writer is at present engaged in an investigation in which a quite different sort of detractor is used,—used, however, in accordance with the main principles established in the following chapters.

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[36] Külpe: "It is clear that the mere employment of distracting stimuli of a certain intensity or number is absolutely no guarantee that a corresponding distraction of the attention has actually been accomplished." *Outlines of Psychology*, 1901, Trans. by Titchener, 429.

[37] Geissler found that even the most complex combination of distractors, after a few days' work failed to induce great variations in attention. *Op.*

SUMMARY

It is necessary in reaction experiments to distinguish sharply between two principal procedures: the first, that in which the same preparatory interval is used a number of times in succession, and change from one preparatory interval to another made in regular order from the shortest to the longest; and the sec-

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cit., 513. More recently Dallenbach has come to the following conclusion: "The difficulty of obtaining a graded series of distractors is very great. In our experience, the action of the distractors is not constant, but varies from day to day, and from observer to observer." *Op. cit.,* 507.

[38] Piéron, *Recherches sur les lois de variation des temps de latence sensorielle en fonction des intensités excitatrices, L'Année Psychol.,* 1914, 17-96.

ond, that in which a number of preparatory intervals of widely different length are mixed in irregular order.

The effect of variation of the preparatory interval on reaction time, when the first procedure is used, may be summarized as follows: Intervals very near 2 secs. are the most favorable. As the interval is shortened below this most favorable region, or lengthened beyond it, the reaction time increases in a marked manner. The increase which occurs with prolongation in the preparatory interval beyond the most favorable is at first rapid, but becomes less and less marked with further increase in the duration of the interval. This increase continues to intervals over 24 secs., and is in accordance with the law, $y = A + B. \log x$, in which y = reaction time, A and B are constants, and x = the duration of the preparatory interval.

With the second procedure, that in which a number of preparatory intervals of widely different length are mixed in irregular order, little difference in reaction time occurs with the different intervals, and the reaction time obtained with each interval depends largely on the order in which the intervals are given and on the individual characteristics of the subject. A noticeable general tendency exists, however, for the longest reactions to occur with the longest and with the shortest intervals of the series, especially however with the shortest intervals, and for the shortest reactions to occur with the intervals of medium length.

An interval of about 2 secs. with the first procedure gives the shortest reaction times for any individual obtainable under any conditions. Even when the same preparatory interval is repeated a number of times in succession, maximal adaptation of attention is impossible if the interval varies much from 2 secs.

The average reaction time for the whole series, using the second procedure, of irregularly mixed and widely differing intervals, is as long as that with a very long (over 24 secs.), regularly repeated interval.

The marked prolongation in reaction time which occurs as we change from a regularly repeated preparatory interval of 2 secs. to a set of widely different irregularly mixed intervals is due solely to the fact that in the latter case the reactions occur with less adaptation of attention to the reaction. We have, therefore, in the method of using an irregularly mixed series of widely different preparatory intervals, a method of securing less than maximal adaptation of attention, that is, a method of detraction from attention. This detraction not only acts solely on the attention, but is eminently controllable, and very uniform in its action on a given subject.

Incidentally, the data of this chapter have an important bearing on the perception of time. Since, with the regular procedure, the increase in reaction time is regular and in accordance with the same law from 2 secs. up through 24 secs., and since the increase in reaction time with this procedure is dependent upon the decrease in the absolute accuracy of the estimation of the intervals, it follows that the absolute accuracy of the estimation of intervals decreases in accordance with a law which holds without break from 2 secs. up through 24 secs. It is consequently incorrect to say of any interval less than at least 24 secs. that it is the longest which can be grasped as a single temporal perception.

CHAPTER III

The Law of Detraction

In the last chapter I arrived at the conclusion that the use of an irregular series of widely different preparatory intervals or of regularly repeated but long preparatory intervals produced a marked prolongation of the reaction time beyond that obtained by the use of a regularly repeated 2 sec. interval; and that this prolongation was due to the lessened average degree of adaptation of attention which occurs as the result of the use of irregularly mixed or long preparatory intervals. The amount of the detraction effect produced by the irregular series of intervals is measured by the increase in reaction time beyond that obtained with a maximum adaptation of attention, that is, with a regularly repeated 2 sec. interval. Having concluded that we have here a satisfactory method of detraction from attention, I naturally took up next the investigation of the behavior of attention under detraction.

A condition could hardly be spoken of as a detractor from attention, unless, other conditions constant, it resulted in lessened efficiency in the processes to which the attention in question attached. Most writers insist that in a detraction method of some sort lies the best hope of obtaining a practical method of measuring attention, provided that suitable detractors (hitherto usually thought of as distractors) can be found. But how the decrease in efficiency, which measures the detraction effect, is to be used in measuring the degree of attention, has, it seems to me, never been worked out. It is true that some law of detraction has often been assumed or implied, though comparatively seldom stated. The implication usually has been that the better the attention the less would be the detraction produced by a stated detractor.[1]

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[1] A good statement of a law to this effect is given by Deuchler in a footnote summing up some of the most important laws of attention. "Je groesser die

That a given detracting condition produces an effect varying inversely as the degree of attention against which it is directed seems fairly plausible, perhaps, but I hesitated to accept a law like this for use in the measurment of attention until it had been experimentally established. It is worth while to know how to prove even that which seems axiomatic, and the law of the relation between the degree of attention and the effect of a given detractor is far from axiomatic. That the matter has never been thoroughly thought out is indicated by the fact that no mention is usually made as to whether equal relative decreases in efficiency or equal absolute decreases, produced by a distractor, would indicate equal degrees of attention; and this point is, of course, of fundamental importance in any practical measurement of attention. Some writers seem to imply that we should take the percentage of decrease, but why should we do so? We wish to use detraction effects as measurements of attention. These effects may be equal relatively and widely dissimilar absolutely, or equal absolutely but widely dissimilar relatively. How then are they to be used as measurements of attention? This question, I believe it will be readily admitted, is one whose answer is not self-evident; and even after the experimental work reported in these chapters, I feel capable of giving an answer that will hold only for a particular kind of procedure, a particular kind of detractor and a particular kind of measurement of the efficiency of the function in which the effect of the detractor becomes noticeable. As I wish at this point merely to indicate that a problem demanding experimental solution exists, and that the problem is an intricate one, I shall reserve my answer to the same until after the presentation of data.

The problem of the relationship between the degree of atten-

Aufmerksamkeits Intensitaet bei der Einengung oder bei der Konzentration ist, desto groesser ist der Widerstand gegen innere oder aeussere Stoerungen." *Beitrage zur Erforschung der Reaktionsformen, Psych. Stud.*, 1913, 223. *Cf.* also Kuelpe, "The attention to a definite content is regarded as inversely proportional to the magnitude of the distraction." *Outlines of Psychology,* trans. by Titchener, 1901, 428. "Distraction" as here used by Kuelpe evidently means the distracting effect produced by a "given" distracting condition or set of conditions.

tion and the effect of detracting conditions is a problem of the relationship between two variables. One of these variables is the degree of attention operated upon, the other is the amount of detraction effect produced by a given detractor. We ought to have, therefore, not only a reliable detractor, which would affect solely the degree of attention, and which would produce a measurable detraction effect, but also different degrees of attention which would be measurable, since we cannot plot a relationship between two variables unless we can measure each. Since my ultimate problem is just that of the measurement of different degrees of attention, it may be readily imagined that it was impossible to have such an ideal set of conditions. The detractor I used was reliable, acted solely on attention and produced very exactly measurable effects; but for the different degrees of attention, I had to be content with ranking them in order.

The different degrees of attention in order to be satisfactory for my purpose had to be produced with steadiness and reliability, so that I could be sure of what the detractor was working on throughout a long series of measurements. The ordinary detractors which are also distractors, such as some simultaneous mental process, would not do for this purpose, i.e., securing steady differences of attention, as it is well known that they are unreliable.[2] The method of obtaining different degrees of attention which I finally decided upon as the first to try out, is one which is very simple. I again resorted to the use of a detractor which was *not* at the same time a distractor. As has already been pointed out, one such detractor, when the task in which attention is involved is a simple reaction, consists in preparatory intervals other than regularly repeated 2 sec. intervals, the best thing being a set of widely different irregularly mixed intervals, though regularly repeated long intervals may also be used, as long as no comparison between individuals is desired. Now there are conditions of attention, in the reaction, other than a 2 sec. preparatory interval. And if the phrase so often used "conditions of attention" means anything, it means that when the conditions of attention are fulfilled, other conditions constant, a higher degree of attention is secured than when they are not fulfilled. Of all the conditions of attention, there is none more generally recognized than intensity of stimulus.[3] In spite of the fact that intensity of stimulus is so universally admitted to be one of the conditions of attention, it is still possible that some will hesitate to accept it at its full significance as such. Intensity is a condition which, as Kuelpe puts it, is primarily valid for the phenomenon of involuntary attention.[4] And the mistake of identifying attention with secondary attention is not uncommon, even with the professional psychologist.[5] But we cannot but admit that intensity of stimulus is a condition of attention and that when other conditions are equal, more or less intensity will bring about a greater or less degree of attention. Consequently, by using different intensities of stimulus in the reaction time experiment we secure different degrees of attention.

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[2] See Titchener, *The Psychology of Feeling and Attention*, 1908, 278.

It is true that the evidence cited in support of the generally accepted statement that intensity of stimulus is a condition of attention consists chiefly in casual observations.[6] We do not know how the degree of attention varies with the degree of intensity, but however this may be, it seems a conservative assumption that differences in intensity great enough to produce very large modifications in reaction time will condition different degrees of attention. It is known that reaction time does not ordinarily vary much with medium ranges of intensity, especially in the case of sound. Wundt explains this relative constancy of reaction time for medium intensities as being due to the constancy of attention,[7] and apparently concurs with Martius, who explains the slowing in reaction time with weak intensities as due to a lesser degree of adaptation of attention.[8] There may, then, be some ground for the belief that the degree of attention is different whenever we have as the result of differences in intensity of stimulus a difference in reaction time. The quickness of reaction to visual stimuli, in turn, up to moderate intensities has been found to vary, roughly, with intensity, in about the same way as apparent intensity varies with variation in intensity of stimulus.[9] In so far then as a parallelism exists between the variation in reaction time with intensity and the variation in

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[3] "Ein intensiver Eindruck wird in der Regel, sofern nicht besondere Dispositionen entgegenwerken, klarer appercipirt als ein schwacher." Wundt, *Physiologische Psychologie,* 5th ed., vol. III, 1903, 340.

[4] "But we also distinguish different *degrees* of attention, according to the intensity of the concentration or the steadiness of its hold upon the various contents. We must, therefore, so far as we can, elucidate the conditions of these degrees of attention. . . . All these conditions including intensity of stimulus are primarily valid for the phenomenon of involuntary attention. But they are also of influence upon the voluntary form of the process, as is shown in the increased ease or rapidity with which the attentive act is accomplished under their direction. And they enable us at the same time to explain the relative vividness, the greater or less degree of attention in the individual case." Kuelpe, *Outlines of Psychology,* trans. by Titchener, 1901, 437.

[5] "This mistake of identifying attention with secondary attention, is especially natural to the man of science, who is always puzzling and searching. So it comes about that psychologists have proposed to measure the degree of attention by measuring the degree of effort, which accompanies it." Titchener, *A Text-Book of Psychology,* 1909, 294.

[6] Titchener writes ". . . the appeal to casual introspection is a confession of scientific weakness, and the remark applies in the present instance. . . . The general dependence of clearness upon intensity of stimulus is an evident

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fact, but it is a fact that we must leave in the rough." *The Psychology of Feeling and Attention,* 1908, 189-190.

[7] Die eroerterte zwischen den Grenzen des Minimal und Maximal-reizes bestehende durchschnittliche Constanz der Reactionszeit zeigt deutlich, dass diese wesentlich von psychophysischen, mit der Aufmerksamkeit im Zusammenhang stehenden Vorgaengen abhaengt, und dass auf sie die Leitungsverhaeltnisse in den Nervenfasern, die, wie die Untersuchung der Leitungsgeswindigkeit zeigt, keineswegs unabhaengig von der Reizstaerke sind, wahrscheinlich keinen erheblichen Einfluss haben." *Physiologische Psychologie,* 5th ed., vol. III, 1903, 430.

[8] "Der Grund der Zunahme der Reactionszeit bei abnehmender Reizintensitaet, wie sie bei sorgloser Ausfuehrung der Reaction ueberall auftritt, liegt in der Schwierigkeit der Perception schwaecherer Eindruecke und in der langsameren Coordination von Eindruck und Bewegung.

Zur Annahme, dass rein physiologishe Gruende in den Leitungsvorgaengen die Reactionszeit der schwaecheren Eindruecke verlangsamen, liegt kein directer Anhaltspunkt in dem Reactionsversuchen vor." Martius, *Philos. Stud.,* 1892, 486.

[9] ". . . the conclusion may safely be drawn that the time of reaction tends to increase arithmetically as the intensity of the stimulus decreases geometrically." Froeberg, *The Relation Between the Magnitude of Stimulus and the Time of Reaction, Archives of Psychol.,* 1907, 15.

the degree of attention with intensity, to an equal extent would a parallelism exist between degrees of attention and apparent intensity, in so far as both were determined by the intensity of stimulus. Now, I should not regard it at all safe to assume anything so definite as a parallelism between the degree of attention and apparent intensity, but believe that enough parallelism exists between reaction time and the degree of attention, in so far as both are dependent upon intensity of stimulus, so that it is safe to assume that a *decrease in intensity sufficient to give a large decrease in reaction time will also condition a lower degree of attention.* That this assumption is correct is clearly demonstrated by the experimental data presented later.

In this connection, it may be pointed out that in view of the fact that the preparatory interval in reaction time may be manipulated in such a way as to constitute a reliable and constant detractor from attention, we have here not only a procedure for determining whether intensity is a condition of attention, but also a method of measuring its strength as a condition of attention. Here is a method suited to a large field of quantitative investigation. We use a fixed detractor from attention, a detractor, it must be remembered, which produces its effect by acting exclusively upon attention, and then vary any other factor we wish. If the variation of this other factor produces variation in the effect of our fixed detractor it is a condition of attention. Its strength as a condition of attention is indicated by the amount of the change variation in it produces in the amount of detraction effect produced by our fixed detractor. I have experimented in this way with respect to intensity of stimulus, and have obtained a result which clearly demonstrates that intensity is a condition of attention.

The procedure, then, for determining how the effect of a given detractor of attention varies with the degree of attention was as follows: Four different intensities of light stimuli were used, and in the case of each intensity the detraction effect produced by unfavorable preparatory intervals was determined. It was assumed that degrees of intensity of stimulus sufficiently different to give marked differences in reaction time would condition different degrees of attention, and that the highest degree of attention would be obtained with the greatest intensity of light. In the selection of the four widely different intensities of light stimulus, the simple reaction time with a regularly repeated 2 sec. interval served as a guide. I tried to obtain intensities which, with a preparatory interval of 2 secs., gave reaction times differing by about equal amounts from each other.

Now while intensity is a condition of attention, it has not been shown that variations in intensity affect no other factor in the reaction than the factor of attention. But the point is that different degrees of attention are secured; and since the detractor used detracts from attention *only,* it would not have a different effect in the case of the different intensities except in so far as it operated upon different degrees of attention. Variation in anything else than attention would not change the effect of the detractor. The application of the given detractor of attention could produce different results in the different cases, only providing it met with different degrees of attention. Whatever other differences than differences in attention might exist, it is the attention component solely that would be acted upon by a detractor which detracts from attention.

The best method of using unfavorable preparatory intervals as a detractor is to use an irregularly mixed series of widely different intervals. But it is also true, that regularly repeated long intervals produce a lengthening of the reaction time, and that the immediate cause of this lengthening is a lessening of the degree of adaptation of attention. The method of regularly repeated long intervals is unsatisfactory as a method of detraction providing we wish to compare the attention of different individuals, because differences in individuals' capacities to estimate long intervals would come in as a serious complicating factor. In the case of a single subject, however, the capacity to estimate intervals may be regarded as a constant factor, or at any rate one which is not likely to change markedly or suddenly with variation in the intensity of stimulus. This method of using long regularly repeated intervals is, therefore, a satisfactory method of detraction in the case of a single individual. The prolongation in reaction time is always due to a lesser degree of attention. If the interval cannot be estimated accurately enough for the subject to know just about when the stimulus is due, he will be less ready to react when the stimulus comes; in other words, the stimulus will meet with a lower degree of attention. In an experiment like the present, which aims at an understanding of the effect of intensity on the detraction effect of unfavorable preparatory intervals, it seemed better to work out the whole curve in the case of each intensity, of the effect of prolonging the preparatory interval, than to use merely one set of irregularly mixed intervals. The effect is in this way shown more in detail.

While I have stated my problem as the measurement of the effect of a given detractor upon four different degrees of attention, with the object of determining how the detraction effect may be used to measure the degree of attention, the study actually made may also be stated simply as one in reaction time. So stated the problem would run: How does the effect of variation in the preparatory interval vary with variation in the intensity of the stimulus? It is the problem of the effect upon reaction time of combined variations in intensity of stimulus and duration of preparatory interval, and is one that has not hitherto been systematically investigated. As will be seen, the effect of the two factors working in combination is not merely the sum of the effect produced when each acts separately.

As regards the apparatus and procedure, the arrangements were similar to those described in the last chapter in connection with Table I, a Hipp's chronoscope being used, with the same storage battery and control apparatus. The stimulus, however, consisted in a light in place of a sound. For the purpose of exposing this stimulus a stereopticon lantern was used. I experimented with the Bergstroem apparatus,[10] but found a stereopticon more satisfactory. The projection screen consisted of a ground glass plate

[10] Bergstroem, *Pendulum Chronoscopes and Accessories for Psychological Experimentation, Psych. Rev.*, 1910, 1-18.

fixed into a small window that had been built into the wall between the dark room in which the subject was placed, and another room in which were the experimenter, the stereopticon and the chronoscope. Felt screens arranged all about the glass plate between the two rooms and about the streopticon cut off all light falling upon the glass plate except that projected through the lenses of the stereopticon. A stereopticon slide was made by pasting black paper entirely over a colorless cover glass, except for a very small square aperture in the middle. At the short distance used between stereopticon objective and the screen, the image of this square when projected upon the screen had a size of 2.2 cms. by 1.2 cms. The exposure of the image was controlled by means of a Seashore tachistoscope, placed in the path of the rays of light, not far from the nodal point. If the shutters of the tachistoscope opened just the slightest bit the image was visible. The shutters of this instrument are held closed by an electro-magnet, and when the circuit through the magnet is broken, pulled open by springs, and in the case of the lower shutter, by gravity also. By attaching light glass pointers to the shutters and having them mark on a kymograph alongside a 250 d.v. fork and a signal magnet, I found that the time required for the upper shutter to open completely was 20σ and for the lower, 14σ: but the time from the break of the current, registered by the signal magnet, till the time when the shutters had opened far enough to expose the image was only 8σ. The exposure of the light was delayed, then, 8σ after the breaking of the electric circuit which held the shutters together. From this constant error 2σ was deducted, as the latter constant was found to be the time elapsing from the moment the break occurred in the tachistoscope circuit to the moment the make occurred in the chronoscope circuit. This left a net constant error in the reaction times of 6σ, which was deducted from all averages obtained. This constant error, while measured with all possible exactness, is probably of little or no importance, in view of the fact that it is only the comparison of reaction times under different conditions that is here in view, not the absolute times.

Since it was desirable to use a different current for the tachistoscope than for the chronoscope, a rigid double switch was used, constructed in such a way that as one half the switch opened the tachistoscope circuit, the other half, insulated from the first, closed the chronoscope circuit.

To obtain different intensities of light, tungsten lamps were substituted as the source of light in place of the carbon arc of the stereopticon. Four different intensities were used, the different intensities being obtained with a 250-watt, or a 40-watt, all-frosted bulb, placed at suitable distances back of the stereopticon lenses. The intensity of the image upon the projection plate was not measured, as the variation in reaction time with intensity was not my problem. The reaction time itself served me as an index of the intensity. I used two subjects with all four of the intensities.

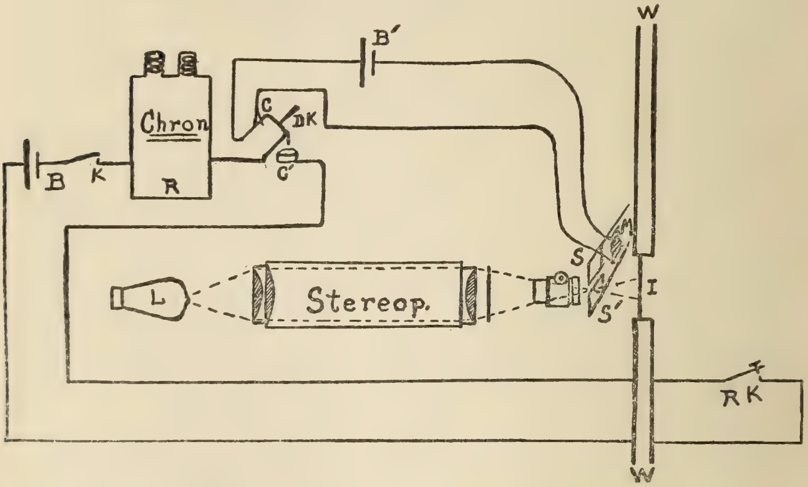
The subject sat 4 ft. away from the image, and straight in front of it, at a table upon which was clamped the reaction key: The room was a fairly "sound-proof" dark-room, but was illuminated, somewhat dimly, by a single 40-watt all-frosted bulb situated about 20 ft. behind the subject. The object of this illumination was to render visible to the subject a black fixation cross drawn in ink upon the ground glass screen at the exact place where the light stimulus appeared. The image appeared and disappeared with all imaginable suddenness, except when the intensity was very faint, in which case it sometimes seemed gradually to grow into its maximal brightness. The subject sat in the dark room for fifteen minutes before beginning to react, so as to become fairly well adapted to the degree of illumination, and thus avoided the risk of a change in the apparent intensity of the light stimulus as the result of changes which might occur in the state of adaptation of the retina. The instructions (one subject was the writer) were for the "motor" form of reaction. This form was chosen for the reasons stated in the last chapter, and in the present instance for the additional reason that both subjects were highly practiced in this form of reaction, and could be expected to stick very closely to a stereotyped form of reaction throughout the entire experiment.

The warning signal was a very short ring of a damped electric bell. The experimenter determined the interval between warning signal and stimulus as accurately as possible by counting the beats of a metronome. All the manipulations of the experimenter were absolutely inaudible to the subject in the dark-room, thanks to the fact that the ground-glass screen was built right into the wall separating the two rooms.

A diagram of the apparatus is given in Fig. 6.

Figure 6

Diagram of Apparatus for Reactions to Visual Stimuli.



B = storage battery.

B′ = origin of the tachistoscope circuit, a circuit shunted off from a 110 volt direct current supplied by the university.

K = key, non-essential, but convenient.

Chron. = Hipp's chronoscope.

R = slide resistance connected in parallel with the chronoscope.

C and C′ = contacts for double key.

DK = double key.

M = electro-magnet controlling tachistoscope shutters.

S and S′ = tachistoscope shutters.

L = tungsten lamp.

Stereop. = stereopticon.

I = oblong image on ground glass plate. This image constituted the stimulus.

WW = wall separating the experimenter from the subject.

RK = subject's reaction key.

The double key, DK, when raised to touch the contact C, closed the tachistoscope circuit, which passed through the electro-magnet, M, holding the shutters SS′ of the tachistoscope closed, and thereby cutting off the image of light at I. The slightest

TABLE VIII

The Effect of Variation in Intensity on the Effect of Variation in the Preparatory Interval, in the Case of Reactions to Light with the Regular Procedure.
Subject Vs.

N, for each interval of each series = 25. Total No. of reactions = 3700.

Interval		1	2	4	8	12	16	20	24	28
Bright...	Series I....	218	211	214	238	265	288	291	299	300
“		*24*	*10*	*20*	*14*	*27*	*20*	*27*	*26*	*29*
“	“ II...	198	193	205	231	247	271	274	292	291
“		*11*	*11*	*11*	*28*	*16*	*22*	*24*	*23*	*29*
“	**Average....**	**208**	**202**	**210**	**235**	**256**	**280**	**283**	**296**	**296**
Dim.....	Series I....	285	287	287	326	354	390	400	406	391
“		*23*	*23*	*19*	*34*	*32*	*61*	*43*	*43*	*49*
“	“ II...	270	284	303	328	375	388	384	403	408
“		*26*	*23*	*25*	*36*	*51*	*63*	*58*	*58*	*49*
“	“ III..	265	280	308	335	383	444	438	438	445
“		*19*	*17*	*27*	*31*	*49*	*76*	*98*	*52*	*70*
“	“ IV...	294	303	301	333	384	397	392	400	402
“		*22*	*35*	*11*	*23*	*52*	*31*	*33*	*36*	*21*
“	“ V....	314	298	328	356	385	413	419	425	438
“		*23*	*15*	*19*	*25*	*43*	*71*	*54*	*48*	*51*
“	**Average....**	**286**	**290**	**305**	**336**	**376**	**406**	**407**	**414**	**417**
Quite dim	Series I....	313	348	370	423	477	467	518	480	471
“		*21*	*36*	*36*	*55*	*95*	*78*	*90*	*83*	*56*
“	“ II...	280	297	333	358	459	452	418	476	485
“		*48*	*38*	*37*	*25*	*72*	*58*	*36*	*41*	*52*
“	“ III..	316	329	330	378	432	500	468	495	501
“		*17*	*15*	*21*	*46*	*54*	*83*	*67*	*69*	*71*
“	“ IV...	315	304	402	405	416	436	449	446	499
“		*23*	*31*	*76*	*56*	*34*	*36*	*64*	*48*	*86*
“	“ V....	320	328	363	435	447	443	527	483	505
“		*35*	*44*	*50*	*41*	*52*	*39*	*88*	*39*	*69*
“	**Average....**	**309**	**321**	**360**	**400**	**446**	**460**	**476**	**476**	**494**
Very dim	Series I....	372	368	398	453	671	611	651	654	...
“		*59*	*48*	*75*	*49*	*141*	*174*	*149*	*140*	...
“	“ II...	355	400	421	625	611	641	696	655	...
“		*47*	*61*	*71*	*111*	*115*	*117*	*127*	*139*	...
“	“ III..	389	426	485	467	472	582	581	595	...
“		*61*	*61*	*81*	*66*	*98*	*111*	*75*	*94*	...
“	“ IV...	325	373	419	484	488	505	458	528	...
“		*27*	*49*	*70*	*78*	*80*	*101*	*52*	*89*	...
“	“ V...	385	365	395	480	513	499	523	543	...
“		*93*	*53*	*73*	*86*	*100*	*92*	*91*	*99*	...
“	**Average....**	**365**	**386**	**424**	**502**	**551**	**568**	**582**	**595**	...
Average for all intensities.............		292	300	325	368	407	429	437	445	...

downward movement of the key, DK, broke the tachistoscope circuit, and made the chronoscope circuit through the contact C′, thus causing the shutters to fly open and the image to appear at I at the same moment that the chronoscope circuit was made. The time between the break at C and the make at C′, however, was long enough to be measurable, and was found to be about 2 σ, already mentioned as a constant error.

With each of the four intensities reactions were taken with preparatory intervals varying from 1 to, usually, 28 secs. Twenty-five reactions were taken in succession with each interval, beginning with the shortest interval and going up to the longest and then stopping for that day. Each Series in Tables VIII and IX gives the results for one day's sitting. The intervals are given in secs. in the first line of the tables, headed Interval. The

TABLE IX

Same as Table VIII, but Subject Ww.

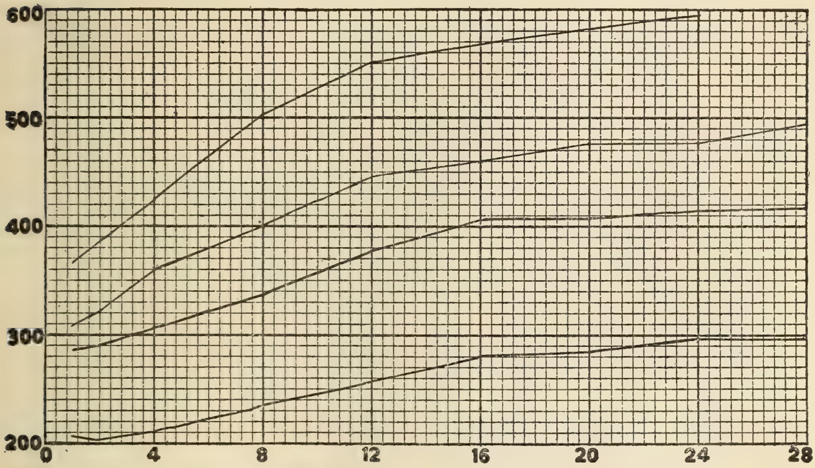
N, for each interval of each series = 25. Total No. of reactions = 1675.

Interval		1	2	4	8	12	16	20	24
Bright.....	Series I.......	197	202	218	228	238	251	266	256
"		*14*	*11*	*14*	*15*	*18*	*23*	*18*	*23*
"	" II......	184	173	198	210	236	243	242	258
"		*16*	*9*	*16*	*18*	*28*	*20*	*25*	*16*
"	**Average......**	**191**	**188**	**208**	**219**	**237**	**247**	**254**	**257**
Dim.......	Series I.......	271	253	270	335	342	376	362	371
"		*32*	*12*	*19*	*25*	*28*	*25*	*21*	*25*
"	" II......	258	244	268	279	300	327	350	353
"		*18*	*14*	*13*	*14*	*30*	*24*	*24*	*20*
"	**Average......**	**265**	**249**	**269**	**307**	**321**	**352**	**356**	**362**
Quite dim..	Series I.......	338	322	356	433	434	462	491	...
"		*30*	*24*	*26*	*42*	*49*	*49*	*53*	...
"	" II......	320	316	330	373	401	434	426	...
"		*35*	*29*	*26*	*38*	*56*	*45*	*40*	...
"	**Average......**	**329**	**319**	**343**	**403**	**418**	**448**	**459**	...
Very dim ..	Series I.......	375	327	436	510	549	578	613	...
"		*36*	*65*	*59*	*53*	*107*	*82*	*113*	...
"	" II......	396	354	428	478	520	555	679	...
"		*58*	*36*	*68*	*93*	*61*	*61*	*125*	...
"	" III......	356	326	416	531	566	595	622	...
"		*54*	*42*	*80*	*95*	*104*	*98*	*80*	...
"	**Average......**	**376**	**336**	**427**	**506**	**545**	**576**	**638**	...
Average for	all intensities..	290	273	312	359	380	406	427	...

average reaction time and the mean variation in σ, are given in each case, the mean variation in italics under the corresponding average. The intensities are stated merely as bright, dim, quite dim and very dim. A more accurate idea of the intensity may be gained by a glance at the reaction time obtained with the 2 sec. interval. The results of Table VIII are shown in graphic form in Fig. 7, and those of Table IX, in Fig. 8.

FIGURE 7

Graphic Representation of Table VIII.



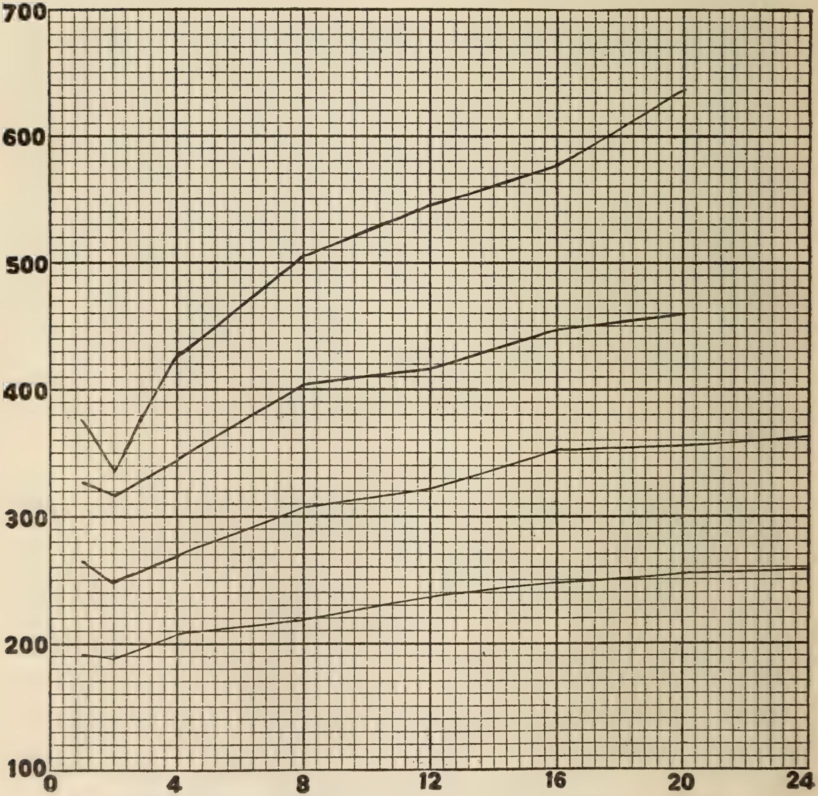
The abscissae represent the duration of the preparatory interval in secs., and the ordinates the average reaction time in σ. Each curve represents the results obtained with one intensity of stimulus, the lowest curve corresponding to the greatest intensity and the uppermost curve to the weakest intensity.

The results presented in Tables VIII and IX and in Figs. 7 and 8 lead to a number of interesting conclusions. Studied merely for the effect of prolongation of the preparatory interval on the reaction time, they show in the case of each intensity the same general effect that has already been described as indicated by the results of Table I (Chapter II). In the case of each subject, with each intensity, the effect of prolongation of interval beyond the most favorable point is to produce a marked and regular increase in reaction time, and this increase occurs at first at a more rapid rate than later. Since the curves with each intensity have the same general form, I have plotted in Fig. 9, separately for each subject, the average of the results obtained with all four intensities. This gives two curves, of a high degree of reliability, showing how interval affects reaction time, taking the average for four different intensities of stimulus. The curves for the two subjects are very similar to each other and also very similar to the curves of Fig. 2, Chapter I, which show the effect of interval on sound reactions for each of three subjects.

The curves for each subject are in accordance with the formula, already arrived at in Chapter I, $y = A + B. \log x$, in

FIGURE 8

Graphic Representation of Table IX.

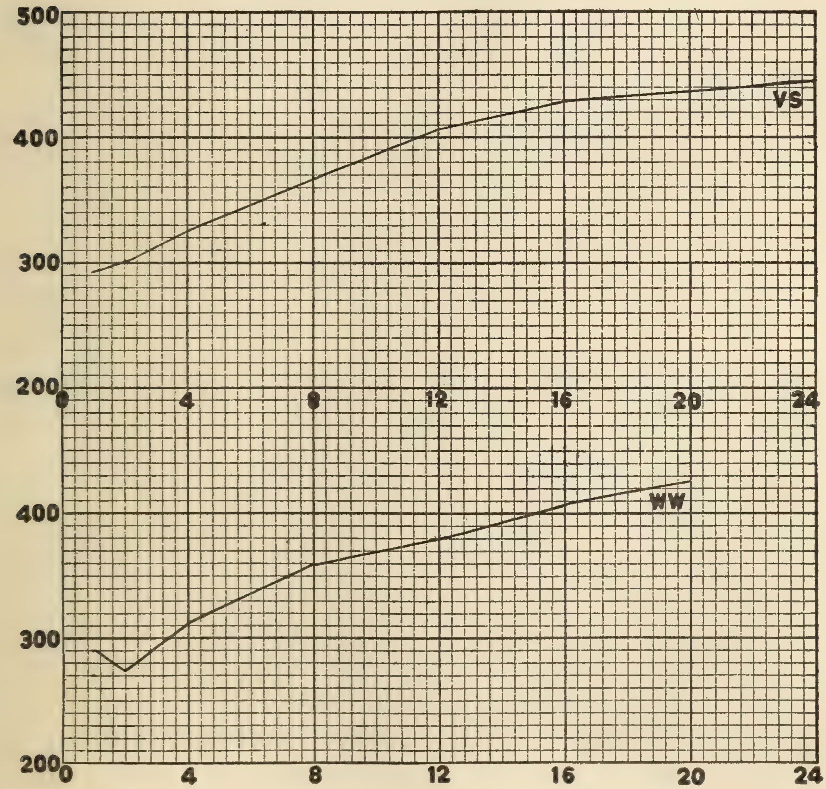


The abscissae and ordinates have the same significance as in Fig. 7.

which y is the reaction time, x is the duration of the preparatory interval, and A and B are constants. In other words, the reaction, for any preparatory interval of 2 secs. or over, is equal to a constant plus the product of another constant into the logarithm of the interval. The deviations of the reaction times experimentally obtained from those calculated in accordance with the equation are shown, for each subject, in Table X. In the

FIGURE 9

Graphic Representation of the Average Reaction Time for All Intensities for Each of Two Subjects.



The abscissae and ordinates have the same significance as in Figs. 7 and 8. The uppermost curve is the curve obtained by combining the four curves of Fig. 7 and the lower curve that obtained by combining the four curves of Fig. 8.

column headed D are given the differences obtained by subtracting the calculated values from those experimentally obtained. R. T. stands for reaction time (expressed in σ).

TABLE X

Subject Vs.

$A = 227\,\sigma$, and $B = 163\,\sigma$.

Interval (x)	Calculated R.T. (y)	Obtained R.T. (y)	D
2 secs.	276	300	+ 24
4 "	325	325	0
8 "	374	368	− 6
12 "	403	407	+ 4
16 "	423	429	+ 6
20 "	439	437	− 2
24 "	452	445	− 7

Subject Ww.

$A = 218\,\sigma$, and $B = 157\,\sigma$.

Interval (x)	Calculated R.T. (y)	Obtained R.T. (y)	D
2 secs.	265	273	+ 8
4 "	312	312	0
8 "	359	359	0
12 "	388	380	− 8
16 "	406	406	0
20 "	422	427	+ 5

It will be observed that the calculated and experimental values differ but slightly, and that the differences between the two values varies irregularly from plus to minus. The one notable discrepancy is in the case of the reaction time with a 2 sec. interval, subject Vs., which, as obtained experimentally, is 24 σ longer than the calculated. But there can be little doubt that the experimental value obtained with this interval really is too long, because it is also longer than the reaction time obtained with this subject with a 1 sec. interval, and there is no doubt that normally the reaction time is shorter with a 2 sec. preparatory interval than with one of only 1 sec. The latter statement is not only generally true, but was found true for this particular subject in the work reported in Chapter II. The fact that in this particular case the reaction time with a 1 sec. interval was shorter than with a 2 sec. interval, I would explain, then, as due to too long a value obtained with the 2 sec. interval, rather than too short a value with the 1 sec. interval. Why the reaction time obtained with the 2 sec. interval is too long, I do not know, but a plausible explanation may be that the subject felt that the reaction was so easy to perform quickly with this length of interval, naturally the easiest, that he did not try as hard as in the other cases.

From the three tables, I, VIII and IX, we may conclude that the effect of the interval upon reaction time follows the same general law whether we use auditory or visual stimuli, and no matter what intensity of stimulus we use. Moreover, the results show that the unfavorable intervals constitute a highly reliable detractor from attention. The same procedure was used day after day, and while it is true that not enough series were taken with any one intensity to determine definitely the effect of practice, no definite decrease in the efficiency of the unfavorable intervals as a detracting condition was noticeable. A special study of the effect of practice is presented in a later chapter, but to be able to take 3700 reactions with one subject without noticing any failure on the part of the long intervals to produce their usual detraction effect is encouraging.

The main object of the experiments described in this chapter, however, was not to determine how reaction time varies with prolongation of the preparatory interval. That was, indeed, sufficiently worked out in Chapter I. But the results included in the present chapter lead to a number of interesting conclusions on other points. The data given in Tables VIII and IX may be looked at in either of two ways. They may be regarded as showing how the prolongation produced by the longer intervals varies with the variation in the intensity of the stimulus, or as showing how the prolongation produced by decreasing the intensity of the stimulus varies with variation in the duration of the preparatory interval. A summary of the data included in Tables VIII and IX from each of these two points of view is given in Tables XI and XIII. Table XI is designed to show how the effect of lessening the intensity of the stimulus varies with prolongation in the preparatory interval, while Table XIII is intended to show how the effect of the long preparatory intervals varies with the intensity of the stimulus.

In the interpretation of each of these tables, I shall follow the same general procedure. I shall first discuss them simply as experiments in reaction time. I shall then discuss them as showing the effects of a given variation in objective conditions upon different degrees of attention. That is, in the case of Table XI, I shall assume that different degrees of attention are brought about by the use of different durations of the preparatory interval, so that I may then consider how the effect of a decrease in the intensity of stimulus upon reaction time varies with the degree of attention; and in the case of Table XIII, I shall assume that different degrees of attention have been brought about by the use of different intensities so that I may then consider how the effect upon reaction time of prolongation of the preparatory interval varies with the degree of attention. And, lastly, I shall consider what conclusions may be drawn by regarding unfavorable variations in the objective conditions as detractors of attention. In all cases I shall consider only the effect of prolongation in the interval beyond 2 secs. The shortening of an interval to below 2 secs. is not strictly comparable in its effect to lengthening it beyond that, and the two sorts of results, therefore, cannot be considered together. As I have used only one interval shorter than 2 secs., I hardly have enough data to make a special study of the effect of shortening the interval to below the most favorable, and this is especially true since the two subjects I used show considerable difference in the relative effect of the 2 sec. and the 1 sec. intervals.

In Table XI, taking the reaction time with the bright stimulus as a standard, I have summarized the effect of decreasing the intensity in the case of each duration of preparatory interval. The preparatory intervals are given in secs. in the first line of the table. Immediately below are given in σ the reaction times with the bright stimulus, that is, the brightest of the four used, for each interval. As an expression of the result obtained by the use of weaker intensities, I have given in the line below, the average of the reaction times obtained with the three weaker intensities. Calling the reaction time for the brightest intensity B, and the average of the times for the three weaker intensities W, the quantity W-B is the absolute prolongation in reaction time produced by the decrease in intensity. As the different intensities were each constants, this decrease in intensity is, of course, to be regarded as a constant. While it is true that the times given in line W are the averages of three weak intensities, I shall speak of the average as that obtained merely by the use of weak intensities, and of the difference W-B as the effect of a given decrease in intensity. The result obtained with the brightest intensity relative to that obtained with the three weakest is shown in the ratio B/W. In so far as the ratios B/W are constant, then, the relative effect of the decrease in intensity is independent of the duration of the preparatory interval.

If in Figs. 7 and 8, which represent by curves the effect of variation in the preparatory interval in the case of four different intensities, one should combine the upper three curves, i.e., those for the three weakest intensities, into one curve, by taking at every point the average height of the three, then the quantities W-B of Table XI would represent the difference between the height of this combined curve and the lowest curve, at each of the points corresponding to the various preparatory intervals used.

A study of the table shows that the absolute prolongation produced by the weaker intensities increases with regularity and in a marked manner with increase in the duration of the preparatory interval. This is shown by the regular increase in the quantity W-B with increase in the duration of the preparatory interval. In Figures 7 and 8 it is shown by the fact that the distance apart of the upper three curves and the lowest increases as they extend to the right.

The increase in the effect of variation in intensity with increase in the duration of the preparatory interval is represented graphically in Figure 10. The two dotted curves show the relation for each of the two subjects individually and the continuous line curve that for the two individuals taken together as a single group. It is true that there is considerable difference between the effects in the two subjects, and yet enough general agreement so that it is worth while to deduce the general tendency shown by both, as expressed in the continuous line curve representing the average result. The formula which seems best to suit this curve is $y = A + B.\ log\ x$, in which y is the increase in reaction time produced by the use of weaker intensities, x the duration of the preparatory interval, and A and B are constants. Taking A as 93 σ and B as 96 σ, the calculated values of y come out very close to those experimentally obtained, as is seen from Table XII in which the values of y are given in σ. In column D are given the differences obtained by subtracting the calculated values of y from the experimentally obtained values.

Table XII shows that the deviations of the experimentally obtained from the calculated values are extremely slight and irregular in direction. The absolute increase in reaction time, then, produced by a given decrease in the intensity of the stimulus increases with the duration of the preparatory interval; the effect of the given decrease in intensity of stimulus increases at first

TABLE XI

(Derived from Tables VIII and IX)

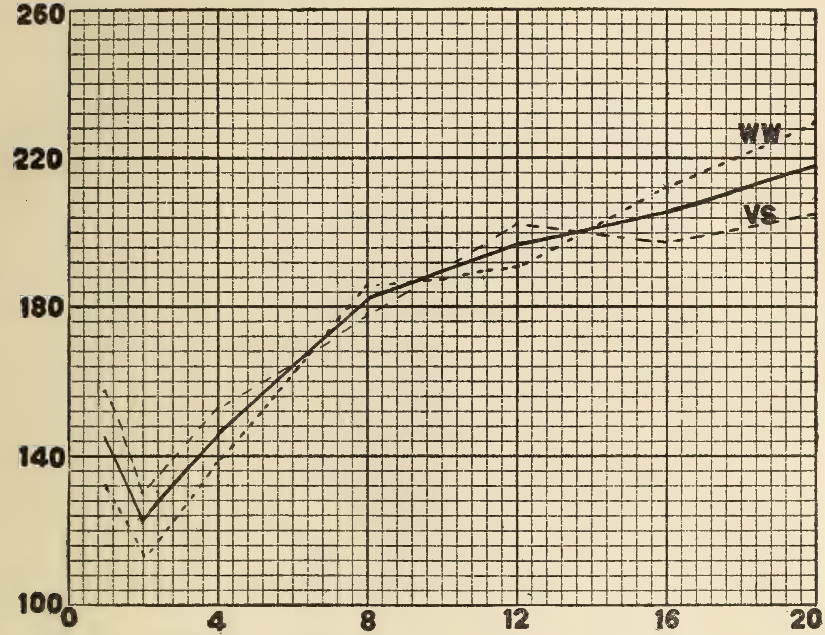
Showing how the Prolongation in Reaction Time Produced by Decreasing the Intensity of the Stimulus Varies with Variation in the Duration of the Preparatory Interval.

Interval		2	4	8	12	16	20	24
Subj.	R.T. (B)	202	210	235	256	280	283	296
Vs	" (W)	332	363	413	458	478	488	495
"	**W-B**	**130**	**153**	**178**	**202**	**198**	**205**	**199**
"	B/W	.61	.58	.57	.56	.59	.58	.60
"								
Subj.	R.T. (B)	188	208	219	237	247	254	...
Ww	" (W)	301	346	405	428	459	484	...
"	**W-B**	**113**	**138**	**186**	**191**	**212**	**230**	...
"	B/W	.62	.60	.54	.55	.54	.53	...
"								

more rapidly than later as the duration of the preparatory interval is regularly increased beyond 2 secs.; and, lastly, stated in a

FIGURE 10

The Variation with Increase in the Duration of the Preparatory Interval in the Increase in Reaction Time Produced by a Decrease in Intensity of Stimulus.



The two dotted curves represent the results experimentally obtained with subjects Ww and Vs. The heavy line curve represents the average result for the two subjects. The abscissae represent the duration of the preparatory interval while the ordinates represent the absolute difference between the reaction time with the brightest intensity and the average reaction time for the three weaker intensities.

TABLE XII

Interval (x)	Calculated y	Obtained y	D
2 secs.	122	122	0
4 "	151	146	— 5
8 "	179	182	+ 3
12 "	197	197	0
16 "	208	205	— 3
20 "	218	218	0

more definite, mathematical way, the absolute increase in reaction time produced by a given decrease in intensity of stimulus increases with increase in the duration of the preparatory interval beyond 2 secs. as a constant plus the product of another into the logarithm of the duration of the interval. The fact that there is a *plus* in the equation indicates, it seems to me, that we should distinguish two effects of the decrease in intensity. In the first place there is a constant effect, corresponding to constant A, one which does not vary with the duration of the preparatory interval, and one which, therefore, does not vary with attention. This may be regarded as the effect of intensity upon factors *other* than attention, all of which remain constant with variation in preparatory interval alone. In the second place there is, in addition, an effect represented by the magnitude *B. log x,* which varies directly as the logarithm of the preparatory interval, an effect, therefore, which varies with the degree of attention, and which becomes greater as the degree of attention becomes less.

As rgards the relation between the reaction time with the weaker intensities and that with the highest intensity, that is, the relative effect of a given decrease in intensity, it will be observed that this tends to remain constant. This is shown by the relative constancy of the ratios B/W in Table XI. This constancy is quite marked throughout for subject Vs, and from the 8 sec. to the 20 sec. intervals for subject Ww. The constancy is not perfect throughout, as in both subjects the ratio for the 2 sec. interval is somewhat larger, and in the case of subject Ww, the ratio for both the 2 sec. and 4 sec. intervals is larger than for other intervals. In the case of subject Vs the variations from the tendency for the ratios to remain constant are slight and irregular, and in view of the fact that the M.V. in the case of the reaction times to even the brightest intensity is about 10 per cent and with the weaker intensities much higher, the deviations from constancy in the ratios is not very reliable. In the case of subject Ww, however, the ratio B/W is decidedly higher for the 2 sec. and 4 sec. intervals than for the others.

If we should, in spite of this deviation in the case of subject

Ww, think of the true tendency as being for the ratios to remain constant throughout, these deviations would indicate that subject Ww had tried harder to react quickly with the short intervals than with the longer ones, in the case of the weaker intensities. The subject, the writer, made no such introspection at the time of the experiment, but the supposition seems plausible. I do not wish to stretch my generalizations, however, and therefore shall not conclude that the true tendency of the ratios B/W is to remain constant throughout. But it is an actual fact that they remain practically constant throughout for subject Vs and from 8-20 secs. inclusive for subject Ww. The general effect of variation in the duration of the preparatory interval, then, on the relative effect of a fixed decrease in intensity may be stated as follows: The relative effect tends to remain constant for intervals varying from 8 to 20 secs., while for shorter intervals the data show a variation. I wish to emphasize the cautiousness of this generalization, and that even though the ratios do not remain constant throughout the whole range of preparatory intervals used, they do remain practically constant throughout a wide range of intervals, a range sufficient to give markedly different reaction times with the same intensity of stimulus. I shall later make considerable use of this fact.

So far the data contained in Table XI have been discussed merely as data upon reaction time. But they have a quite obvious bearing upon the subject of attention, for as has already been pointed out at length in Chapter II, the prolongation in reaction time which occurs with prolongation in the preparatory interval is due to the lesser degree of attention in the case of the reactions following the longer intervals. The difference in quickness of reaction with the different preparatory intervals, under otherwise constant conditions, is due to the difference in the average degree of attention obtaining at the moment of the reaction in the case of the different intervals. Consequently the experiment may be regarded as a study of the effect of a fixed decrease in intensity upon different degrees of attention. All other factors than attention remain constant when the preparatory interval is the only condition which is varied. The sole effect, that shows in the reaction times, which is produced by the variation in interval, is a variation in the degree of the subject's attention. Now, if intensity is not a condition of attention, that is, does not affect the degree of attention, its absolute effect will not vary under conditions which remain constant except for variation in the degree of attention. This holds no matter what other factors than attention may be affected by intensity; for a fixed factor, namely, a given decrease in intensity, would be working upon other fixed factors and the result should be always the same. It is perfectly evident that, if we should assume that a change in the intensity of the stimulus does not affect the degree of attention, then, as long as no other factor than the degree of attention is changed, we should expect a given change in intensity to continue to have the same absolute effect. But, on the other hand, if we find that the absolute effect of a fixed variation in intensity varies markedly with variation in the degree of attention, when other factors are constant, evidently the variation in intensity is affecting the degree of attention. And if the degree of attention is affected by variations in intensity, intensity is a condition of attention. Now it has already been pointed out that the absolute effect of the fixed decrease in intensity increases in a marked manner with increase in the duration of the preparatory interval, in other words, with decrease in the degree of attention, and in accordance with a definite law. Since, therefore, the effect of a fixed variation in intensity varies inversely with the degree of attention, the degree of attention is affected by the variation in intensity, and consequently intensity is a condition of attention. And as a condition of attention its importance naturally increases as the degree of attention conditioned by other factors decreases. We have here then an experimental demonstration of the common assumption that intensity is a condition of attention. The ordinary statement is based upon the direct introspection that attention to a stimulus varies with its intensity, whereas the above experiment demonstrates this indirectly on the assumption that variation in the preparatory interval produces a variation in the degree of attention at the time of the reaction. Consequently, by reasoning backwards, it will be seen that if it is admitted that intensity is a condition of attention, this admission is strong confirmation of the statement that large variations in the degree of attention at the time of reaction are produced by variations in the duration of the preparatory interval.

From the preceding discussion, the conclusion results that a decrease in intensity brings about a decrease in the degree of attention. We may therefore, interpret the preceding experiment as a study in the effect of a detracting condition, namely, weak intensity of stimulus, acting upon different degrees of attention, these different degrees of attention having been produced by different durations of preparatory interval. Although, as I have already shown, variations in intensity affect other factors than attention, nevertheless, since under the conditions of the experiment these other factors were constant, it follows that the variations in the effect produced by the decreases in intensity may be regarded entirely as variations in their effect as detractors of attention. As has already been pointed out, the absolute effect of the variation in intensity increases with decrease in the degree of attention, the latter decrease being produced in the present instance by long preparatory intervals. We may consequently conclude that under the conditions of the present experiment the absolute effect, that is the absolute decrease in efficiency, produced by a constant detractor of attention varies inversely with the degree of attention detracted from. On the other hand, the relative prolongation in reaction time produced by a fixed decrease in intensity does not always vary with variation in the preparatory interval, but on the contrary remains constant throughout a considerable range of intervals, so that we may conclude with certainty that the relative effect of a given detractor *may* remain the same with variation in the degree of attention. It not only may remain the same but shows a rather general tendency to do so, though my results hardly justify the conclusion that it always does so. Now, since we may have variation in the degree of attention without change in the relative effect of a given detracting condition, and since, on the other hand, the absolute detraction effect of a given detractor varies inversely with the degree of attention, we may draw the practically important conclusion, that in the use of a detractor under conditions such as described above, for the purpose of measuring attention, it is the absolute effect which is significant, and not the relative effect. The degree of attention is measured (ranked) by the reciprocal of the absolute detraction effect.

This result is confirmed by other results I have obtained, where I have used other methods of obtaining different degrees of attention, e.g., the use of alcohol, difference in age and having the subject perform simultaneous tasks. These are discussed more in detail in Chapter V, but show that where we presumably are dealing with widely different degrees of attention we still often get very nearly the same relative detraction effect but quite different absolute detraction effects, from given detracting conditions.

So far the data of Tables VIII and IX have been considered as showing the effect of variations in interval on the effect upon reaction time produced by a fixed decrease in intensity. The data acquire additional meaning by studying them as showing the effect of variation in intensity on the effect of prolongation of the preparatory interval. For this purpose I have constructed Table XIII. The first line of the table, headed to the left Intensity gives the intensities used. Line A gives the reaction time for the 2 sec. interval. Line B gives the average of the reaction times for all the longer intervals used with the subject in question, i.e., intervals from 4 to 24 secs. inclusive in the case of subject Vs and from 4 to 20 secs. inclusive in the case of subject Ww. This average is an expression of the effect of the longer intervals all taken together. The difference between the average reaction time for the longer intervals and the reaction time for the 2 sec. interval, given in the line headed B-A, is the absolute amount of the average prolongation produced by the unfavorable intervals. In the line headed A/B, the ratio of the reaction time with a 2 sec. interval to the average reaction time with the longer intervals is given. These ratios, then, serve to show how the relative prolonging effect of the unfavorable intervals varies with the intensity of the stimulus. All reactions are given in σ.

Table XIII

(Derived from Tables VIII and IX)

Showing how the Prolongation in Reaction Time Produced by Unfavorable Preparatory Intervals Varies with Variation in the Intensity of the Stimulus.

Intensity		Bright	Dim	Quite Dim	Very Dim
Subj.	R. T. 2 secs. (A)	202	290	321	386
Vs	R. T. long (B)	260	374	436	537
"	**B-A**	**58**	**84**	**115**	**151**
"	A/B	.78	.78	.74	.72
Subj.	R. T. 2 secs. (A)	188	249	319	336
Ww	R. T. long (B)	233	321	414	538
"	**B-A**	**45**	**72**	**95**	**202**
"	A/B	.80	.78	.77	.62

Line B-A shows that the absolute prolonging effect of the unfavorable intervals increases very markedly with decrease in intensity. In Figures 7 and 8 this is shown by the fact that the curves representing the results with the weaker intensities show greater increase in height as they extend to the right than the curves representing the higher intensities. It is the rise in the curve, that is, the prolongation produced in the reaction time by the unfavorable intervals, that is numerically represented in Table XIII, column B-A, for each of the four intensities.

The relative prolongation produced by the longer intervals, or the relation of the reaction time with the longer intervals to that with the 2 sec. interval, expressed by the ratio A/B, does not show nearly such marked variation with variation in intensity as does the absolute prolongation. Indeed, with the exception of the weakest intensity, the ratio A/B shows very little variation. The relative prolonging effect of a given unfavorable variation in the preparatory intervals may and does remain constant for a considerable range of variation in intensity (exactly so in the case of subject Vs, for the two highest intensities), but does not do so throughout. The conclusion is uncertain, except when stated to the effect that the relative prolongation in reaction time produced by unfavorable intervals *may* not appreciably vary with considerable variation in intensity.

The arrangement of Table XIII as of Table XI, readily brings out the bearing of the data of Tables VIII and IX upon attention. Since the prolongation in interval acts as a condition unfavorable to attention, and solely for this reason produces a prolongation in reaction time, it may be spoken of as a detracting condition, or a detractor. This detractor, used with four different degrees of intensity of stimulus, produces four different amounts of absolute prolongation in the reaction time. Consequently, it has acted on four different degrees of attention, and these different degrees of attention have been conditioned by different degrees of intensity. The detraction effect produced by the prolongation in interval varies greatly with the variation in the intensity of the stimulus. From this fact we may conclude that the degree of attention varies with variation in the intensity of the stimulus and presumably varies greatly. Consequently intensity is not only a condition of attention, but we may further particularize to the extent of saying that the four intensities used in this experiment produced four different, and quite plausibly, four widely different, degrees of attention.

The absolute effect of variations in attention, then, produced in the above instance by variations in interval, is greater the weaker the intensity of the stimulus attended to. This result, merely from the point of view of experimental technique, is important, as it shows the desirability of using rather weak intensities if one wishes to study the effect of attention upon reaction time, as so many investigators have done. If one wished, for instance, to determine whether an "attention wave" were demonstrable by means of reaction time, one should use a weak intensity of stimulus, as variations in attention will then produce a much more marked absolute effect upon the reaction time. Similarly, one could expect that differences in the degree of attention among the members of a class of school children would be more noticeable after a certain amount of fatiguing labor than early in the morning when they are fresh, as fatigue would act as a detractor something after the fashion of weak intensities in the above experiment. And just as weak intensity magnifies, as it were, the differences in attention due to differences in preparatory interval, so would fatigue be likely to magnify the differences in the attentions of different individuals, no matter to what these differences might be due. The weak attentions would be further weakened a great deal by any given detractor while the good attentions would be weakened only slightly, so that the result of the detractor would be greater absolute difference between the weak and the good attentions.

Since the four different degrees of intensity condition four different degrees of attention, and since the prolongation in reaction time produced by the unfavorable intervals may be regarded as a detraction effect, we arrive again at the conclusion that the absolute effect of a given detractor varies inversely as the degree of attention, while the relative effect may or may not remain constant. The practically important point concerning the relative detraction effect is that it certainly *may* remain constant in spite of variation in the degree of attention; and consequently, as already emphasized, it is the absolute and not the relative prolongation, produced by a given detractor, which is to be taken as an index of the degree of attention.

In conclusion, I wish to argue on theoretical grounds for the plausibility of this result, that it is the absolute detraction effect of a given detractor which is to be taken as a measure of attention, in accordance with the law that the attention varies inversely with the degree of the absolute detraction effect—that the relative effect on the other hand may remain very nearly constant for widely different degrees of attention. A given detractor of attention acts *as such* on attention only, and on no other factor; but the process in which the decrease in efficiency produced by the detractor becomes noticeable, in the present instance a reaction process, is bound to involve other factors than attention, no matter how important the factor of attention may be. Variation in these other factors may produce great variations in the efficiency. The efficiency is determined only in part by attention. And if attention may play at one time a relatively small part and at another time a relatively large part, and yet the absolute degree of attention remain the same, evidently the same amount of interference with attention in the two cases, non-attention factors not being disturbed, could not possibly produce the same effect upon the two efficiencies. Theoretically, it might not produce equal absolute effects, but would do so if attention operated as a factor which was variable independently of other factors. Now attention may be varied independently of other factors, as in the case of variation in interval, where factors involved in the reaction process other than the degree of attention remain constant. Evidently, then, it is the absolute detraction effect and not the relative which should be used in the measurement of attention.

SUMMARY

The effect of prolongation of the preparatory interval beyond the most favorable interval, using the regular procedure[11] is to produce a marked and regular increase in reaction time, and this increase occurs at first at a more rapid rate than later, as the interval is uniformly increased in duration. If we call the reaction time y and the duration of any interval of 2 secs. or over x, we may write the equation $y = A + B. \log x$, in which A and B are determinable constants. This conclusion is identical with a conclusion stated in the summary to Chapter I and based on the results presented in that chapter on reactions to sound stimuli. The effect of variation in the preparatory interval upon the reaction time, therefore, follows the same general law whether we use auditory or visual stimuli. The same law holds for any degree of intensity of stimulus. Of course the constants A and B will vary with different individuals and with different objective conditions.

As regards the effect of the duration of the preparatory interval on the prolongation produced in reaction time by a given decrease in intensity, the data of this chapter lead to the following law:

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[11] See Chap. II, p. 40.

The absolute increase in reaction time produced by a given decrease in intensity of stimulus, increases as the duration of the preparatory interval is increased beyond 2 secs., and this increase takes place at first more rapidly than later, as the interval is uniformly increased in length. Stated more definitely, if y represents the increase in reaction time produced by a decrease in the intensity of stimulus, and x the duration of any preparatory interval of 2 secs. or over, then $y = A + B. \log x$, in which A and B are determinable constants. This law shows that the increase in reaction time produced by a decrease in intensity may be regarded as made up of two factors, one of which will vary with variation in the duration of the preparatory interval, and hence with variation in the degree of attention, while the other will remain constant.

As regards the absolute prolongation in reaction time produced by increase in the duration of the preparatory interval beyond 2 secs., it may be said that this prolongation increases markedly with decrease in the intensity of the stimulus. Since I did not measure the intensities of the stimuli I have not attempted a mathematical formulation of this law.

The relative increase in reaction time produced by a given decrease in intensity may not vary with prolongation in the preparatory interval beyond 2 secs. Similarly, the relative increase over the reaction time with a 2 sec. interval obtained with a longer preparatory interval, may remain the same for widely different intensities of stimulus.

In Chapter I, the conclusion was reached that the effect of variation in the preparatory interval upon reaction time, is due solely to the effect of variation in the interval upon the degree of adaptation of attention; so that the different reaction times obtained with the different intervals correspond to different degrees of attention. Now since the data show that the absolute effect of a given decrease in intensity varies with the duration of the interval, it follows, in accordance with the laws already stated, that the effect of variations in intensity varies with the degree of attention. And any factor which has a greater or less effect on efficiency as the degree of attention involved is greater or less, is a condition of attention. Consequently, intensity of stimulus is a condition of attention. Similarly, since the unfavorable intervals may be regarded as detractors solely of attention, and since their effect was found to vary with intensity of stimulus, we may conclude that intensity of stimulus is a condition determining the effect that a given detractor of attention will produce, in other words, that intensity of stimulus is a condition of attention. The data of this chapter, therefore, assuming the conclusions of the preceding chapter, constitute an experimental proof that intensity is a condition of attention, and that different degrees of intensity condition different degrees of attention. What correspondence between intensity and degree of attention exists, is of course positive, not inverse. Data which corroborate this conclusion are given in Chapter IV.

Now, since the absolute increase in reaction time produced by the use of unfavorable preparatory intervals increases with decrease in the intensity of the stimulus, and since variation in the intensity of the stimulus results, as just pointed out, in corresponding variation in the degree of attention, we may conclude that the absolute increase in reaction time produced by the use of unfavorable preparatory intervals varies inversely with the degree of attention. Or, *the degree of attention varies inversely with the absolute prolongation produced by unfavorable intervals.* In other words, attentions may be measured or ranked, by equating them with the reciprocal of the absolute (but not the relative) prolongation in reaction time produced by the use of unfavorable preparatory intervals. We may use any two given conditions of intervals, one more favorable than the other, as a 4 sec. interval and a 20 sec. interval, both regularly repeated, or a 2 sec. interval regularly repeated and a given set of irregularly mixed intervals. The difference in the reaction times obtained under the two chosen conditions will vary inversely with the degree of attention.

By regarding the effect of unfavorable intervals and of weak intensities upon attention as typical of the effect of detractors in general, we may state the following *general law of detraction: The absolute detraction effect of a given detractor of attention varies inversely with the degree of attention upon which the detractor acts.* This law not only furnishes the basis for the measurement of attention but gives a description resting upon experimental data of one of the most important functions of attention, namely, the increase in efficiency which results from the resistance to distracting or detracting conditions. It points out that attention varies inversely as the ability to resist conditions that are unfavorable to attention and that this ability is measurable in its effect upon efficiency. Indirectly, then, increase in efficiency is a function of attention. Were it practically important to have an absolute measure of attention instead of a relative one, I believe that we could very well substitute increase in efficiency for the function usually regarded as the most essential, namely, clearness, and instead of equating the absolute degree of attention with clearness equate it with the absolute increase in the reciprocal of efficiency produced under specified conditions by a specified detractor.

The expression "absolute detraction effect," in the law of detractors that I have just stated, is necessary, for I have shown that the relative effect cannot be used for the purpose of measurement of attention. The relative effect may remain constant while attention varies, as in the case of the relative prolongation of reaction time caused by the use of unfavorable intervals as detractors, where the relative prolongation remains constant while the degree of attention is varied by varying the intensity of the stimulus.

The phrase "a given detractor" conceals many difficulties, but the conditions determining a satisfactory detractor as well as an analysis of the conditions which must be complied with if the detraction effect is to be adequately measured has already been given in the introductory chapter.

CHAPTER IV

THE MEASUREMENT OF ATTENTION BY REACTIONS TO A CHANGE IN INTENSITY

The general subject of this chapter is the value of reactions to a change in intensity for the measurement of attention. More specifically, the object of the experiments reported in this chapter is twofold. The first set of experiments was made with the purpose of determining whether it is possible to find a form of reaction the time of which is unaffected by variations in retinal sensitivity, and so to get a method of measuring attention apart from its dependence upon retinal sensitivity. The second set of experiments here reported had for its principal object the verification of the law of detraction, as stated in the last chapter, namely, that the absolute effect of a given detractor varies inversely as the degree of attention detracted from; but in the present instance the different degrees of attention were not produced by change in the absolute intensity of the stimulus, as in the preceding chapter, but by changes in the size of the change in intensity, the reaction being to the change in intensity.

The sensitivity of the sense-organ, whether the eye or ear, would quite certainly affect the result of the measurement of attention by the method so far suggested. Decreasing the sensitivity of the sense-organ is the equivalent of decreasing the intensity of the stimulus, and since intensity is a condition of attention this would certainly affect the result. Consequently, by the method used in the last chapter, a person with a low retinal sensitivity as regards intensity would be very likely to have very bad attention, and a person with normal vision, comparatively good attention. For certain purposes such a result would be unsatisfactory. It seems desirable for the purpose of comparing the attention of different individuals to be able to measure an individual's attention, as it occurs in the reaction process, independently of the variations which might be produced in that attention as the result of variation in the condition of the sense organ involved.

Individuals' attentions under the conditions of the reaction experiment, vary in part because of variation in the sensitivity of their sense organs, but there is nothing indefinite in the concept of measuring these attentions in so far as they differ only because of other factors than the factor of sensory sensitivity. Retinal sensitivity, or any other sensory sensitivity, is capable of separate measurement, possibly, and a correction for it might perhaps be worked out, but this would be too difficult and uncertain. I accordingly sought a method of ranking attentions considering them as different only to the degree they would be different as the result of all conditions other than sensory sensitivity.

Simple reaction times, such as I used in the experiments so far reported, are evidently not suited to my purpose. Since the result of any detracting influence, e.g., the unfavorable intervals, varies with the intensity of the stimulus, it presumably varies also with the sensitivity of the retina. We know that increasing the sensitivity of the retina, as for instance by adaptation to darkness, has the same effect as increasing the intensity of the stimulus, that is, results in an increase in apparent brightness; and while I have not experimented in the way reported in the last chapter with different retinal sensitivities substituted for different intensities of stimulus, there is every reason to believe that the effect produced by unfavorable intervals would vary with variations in retinal sensitivity in a way corresponding to that in which it varies with variation in intensity of stimulus.

A single visual stimulus, then, undoubtedly has the effect of its intensity reduced by a decrease in the sensitivity of the retina. But what is the effect of a decrease in sensitivity on the apparent difference in intensity of two light stimuli? The variation in sensitivity of the retina in different people may be due to various causes, but the result here considered is simply variation in the effect of the intensity of the stimulus. In so far as these variations in the eyes of different people do not affect the intensity effect of the stimulus, they need not here be considered. As regards the effect of intensity, it seems to me that there can be little doubt that the result of a decrease in eye sensitivity would be the same as that produced by placing some sort of a translucent screen before the eye, such as a smoked glass or uniform gray film. If the effect of a decrease in the sensitivity of the eye may be thus likened to that produced by holding a piece of smoked glass before the eye, what effect would a decrease in sensitivity produce upon the apparent difference in intensity of two stimuli? A piece of uniform gray glass (produced by photographic methods) will decrease the absolute intensity of both light stimuli but will not change their relative intensity. And by Fechner's law, as long as the two stimuli have the same relative intensity, the difference in intensity of sensation, that is, the apparent difference in intensity, remains constant. Were Fechner's law true, the noticeability of the difference in intensity of the two stimuli would be unaltered by a change such as produced by the interposition of gray glass, or consequently, I think we may assume, by a decrease in the sensitivity of the sense-organ.

It is pretty well established that neither Weber's law nor Fechner's law hold for very weak intensities, as the relative difference limen becomes larger with such. Consequently, if an individual were nearly blind, a given difference in intensity would be harder for him to discriminate than for a normal person. If, however, apart from extreme cases, the difficulty of discrimination is not affected by variation in sensitivity, a discrimination reaction would be quite satisfactory for the purpose now under consideration, as the number of cases of retinal insensitivity so great as to cause a reduction in the effect exerted upon the optic tract by a moderately bright light down to that exerted by a light so faint as to fall below the range of intensities covered, in the case of a normal retina, by Weber's law, is probably, relatively speaking, not great. For such extreme cases of retinal insensitivity a different method of measuring attention must needs be used, perhaps an auditory method.

While the general consensus of psychological opinion appears to be that Weber's law undoubtedly holds good for moderate intensities, there seems much diversity of opinion as to whether a similar law holds for supraliminal differences. It is not my intention to take up here the criticism of the vast literature bearing upon Fechner's law. The experiments so far made are undoubtedly somewhat inconclusive. The explanation for this is that the law for supraliminal differences can not be directly tested except by the use of the method of mean gradation, and this method is a very treacherous one, as Mueller has pointed out.[1] The law governing the noticeability of supraliminal differences is involved in the present discussion, however, only in so far as it determines whether or not the noticeability of the difference between two intensities bearing a fixed ratio to each other is changed sufficiently as the result of change in absolute intensity to produce a different degree of attention to this difference. This needs some explanation. In the case of attention to a single visual stimulus, the intensity of the stimulus, as was shown in the last chapter, is a condition of attention. In such a case it is perhaps in accordance with prevailing psychology, to think of sensory clearness as the criterion of attention and one can readily understand that this clearness will increase with increase in intensity of the stimulus. Different degrees of intensity, therefore, would condition different degrees of clearness of the single sensation. But the case is different if we are attending to a difference, or to a change, in intensity. It is very doubtful that the degree of attention to a difference in sensations is conditioned by the absolute intensities of the two stimuli. If Fechner's law were true, it would seem rather that the degree of attention to the difference would be conditioned by the ratio of intensities. As long as the ratio of intensities remained the same, the difference in intensity would, other things equal, remain equally clear. Now, while absolute intensity is a condition of attention to a single stimulus, it seems that within a certain narrow range of moderate intensities, the intensity may vary without variation in reaction time and so, in all probability, without variation in the degree of

[1] G. E. Mueller. *Die Gesichtspunkte und die Tatsachen der Psychopyhsischen Methodik,* 1904, 234-244.

attention. So, similarly, it is possible that the degree of relative difference in intensity, even though a condition of attention to the difference, may vary within moderate limits without this variation producing a variation in the degree of attention given to the difference. Variation in the degree of difference is probably a condition of attention to difference, just as variation in absolute intensity of a single stimulus is a condition of attention to the stimulus. I have already shown that the variation in intensity is a condition of attention to the single stimulus and I shall later present figures to prove in a similar way that variation in the degree of difference is a condition of attention to the difference. But in both cases, it is possible, and even indicated, that within quite moderate limits variation does not appreciably affect attention. In the case of a single intensity these limits are indicated as those including what we call moderate or moderately strong intensities. In the case of a difference in intensity, they are perhaps those including what we would call easily noticeable differences, that is, roughly speaking, moderately large differences. Consequently, if we are experimenting with rather large differences, it is quite possible that even though as the result of the interposition of a gray plate between the eyes and the stimuli, the degree of apparent difference were slightly altered, that this variation might not be large enough to condition an appreciably different degree of attention to the difference in question. If Fechner's law held exactly, the apparent difference in intensity would remain exactly the same. For our purpose, however, it is perhaps not necessary that the difference remain exactly the same. It is only necessary that it shall not change sufficiently to condition an appreciably different degree of attention. I have, therefore, not attempted to demonstrate Fechner's law, but merely to determine whether the degree of attention to a given change in intensity is varied by placing a gray glass between the eyes and the stimulus. Since I have assumed this effect of a gray glass screen to be similar to that of a decrease in the sensitivity of the eye to intensity, I shall interpret my data as bearing upon the question whether or not a change in the retina's sensitivity to intensity will bring about a change in the degree of attention to a given large difference in intensities.

While, as I have said, no attempt was made to determine the law of supraliminal differences, the results obtained tend to confirm the statement that equal relative differences are equally noticeable, and I believe the same general technique that I used is better adapted to settling this point than is the method of mean gradation. It was assumed that as the difficulty of discrimination varies, there is a variation in the corresponding discrimination reaction times.[2] The data presented in the second set of experiments reported in this chapter show this to be true. The variation in reaction time with variation in the apparent difference is especially marked when the apparent difference is small. It seems likely therefore, that by taking discrimination reaction times with a small difference in the intensities of the stimuli, and by observing whether or not the reaction times vary as the absolute intensities of the stimuli are varied while their relative intensity is kept constant, that a more reliable test of Fechner's law can be made than by the method of mean gradation. The comparison of two differences in intensity, which is necessary by the method of mean gradation, and which is often so uncertain and apt to be based on misleading criteria, is by the method I have described entirely eliminated.

In the experiment next to be reported, however, a small difference in intensity was not used, but one that was noticeable without the least hesitation. The stimulus consisted of two oblongs or bars of light of equal size and brightness projected from a Mazda tungsten lamp upon a ground-glass screen, the general arrangement being similar to that used in the experiments reported in Chapter III. Either the right or left bar of light was suddenly decreased in intensity, and the subject's reaction consisted in calling as quickly as possible into a Cattell voice-key the word "right" or "left." The subject's task, then, a little more specifically, was first to fix definitely in mind which bar of light was the right and which was the left. Both bars being visible before the warning signal (a damped electric bell), no fixation point was necessary. Upon hearing the warning signal the subject, with his mouth already at the voice-key, fixated the two bars and perfected his adjustment to the voice-key at the same time. When the change in intensity occurred, there was no comparison made of the brightness of the two bars. This is certain both from introspection and also from some objective data included in the second set of results reported in this chapter. The decrease in intensity was noticed as a sort of flicker. It would probably be misleading even to say that there had been any real comparison in the case of the bar of light which was decreased in intensity, between its original intensity and the intensity it had after it was decreased. The sudden change in intensity was merely noticed, the reaction word "right" or "left" following as quickly as possible. The reaction therefore was rather a recognition reaction than a discrimination reaction, consisting in recognizing the spacial position, as right or left, of a sort of "flicker sensation" produced by the decrease in brightness of one of the bars. After the reaction, the subject might occasionally make a careful comparison of the brightness of the two bars. From this description of the reaction, it seems that the use of two bars of light in place of one was unnecessary for my purpose, but this in no way invalidates the experiment. That no real comparison was made, is of no particular concern, as the principal object was to find a form of reaction which does not vary with the sensitivity of the eye to brightness.

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[2] "The more difficult the discrimination, i.e., the more nearly alike the stimuli, which must be held apart in reaction, the slower is the reaction." Ladd and Woodworth, *Elements of Physiological Psychology*, 1911, 490. The conclusion quoted is based chiefly upon the results of Henmon, in *The Time of Perception as a Measure of Difference in Sensation,* 1906.

The apparatus was the same as that described in the preceding chapter, except that the lantern slide, instead of having one aperture, had two oblong ones, standing alongside of each other. These oblong apertures produced oblong images or bars of light having a size upon the glass projection screen of 1 cm. by 0.25 cm., with a distance of 0.5 cm. between. The tachistoscope instead of being allowed to cut off the light entirely, was arranged to decrease the intensity of one of the bars without affecting the other. This was accomplished by removing the lower shutter entirely and attaching to the upper one a short strip of glass of uniformly developed gray. The height of the tachistoscope was adjusted so that when the upper shutter was drawn down by the magnet, not the shutter but only the glass strip came in front of the light. The tachistoscope was supported so that it could be easily adjusted to have the glass strip come over either one of the two bars of light. The order in which the right and left bars were covered by the strip was a pure chance order, different for every series of reactions, and unknown to the subject. In order to have the tachistoscope and the chronoscope circuits separate, a double key was used, as in the apparatus described in the preceding chapter, one-half making the tachistoscope circuit and the other half simultaneously making the chronoscope circuit. Thus the decrease in the intensity of the light occurred simultaneously with the close of the chronoscope circuit. A subtraction was made for the constant error due to the latent period of the tachistoscope shutter. The subjects were the writer and two advanced students, all three well practiced in reaction experiments. The subject sat in the dark-room four feet in front of the light stimuli. The dark-room was dimly illuminated from behind the subject.

One of the most essential points in the arrangement of the experiment was to vary widely the absolute brightness of the bars of light without varying the relative decrease in brightness produced by the photographically prepared glass strip. Four different degrees of brightness were obtained by the use of either a 250-watt or a 40-watt lamp, each of which was used both in a position directly behind the stereopticon and at a distance of four and a half feet behind this position. This gave four widely different intensities, which, however, I did not measure. In Table XIV the intensities are referred to simply as medium, weak, weaker, and very weak. They were not identical with the intensities used in the experiments of the preceding chapter. The glass strip attached to the tachistoscope shutter would evidently cut off the same percentage of light in all cases. Twenty-five reactions were taken with each intensity before passing to the next. The intensities were taken in order from brightest to weakest and from weakest to brightest, back and forth three or four times. A preparatory interval of 2 secs. was used throughout.

Table XIV

The Effect of Variation in Absolute Intensity upon the Time of Reaction to a Constant Degree of Change in Relative Intensity.

N, for each series with each intensity = 25. R.T. = average reaction time in σ.
Total no. of reactions = 1,825. M.V. = mean variation.

Intensity		Medium		Weak		Weaker		Very Weak	
Subject		R. T.	*M. V.*	R. T.	*M. V.*	R. T.	*M. V.*	R. T.	*M. V.*
Vs	Series I......	424	*53*	494	*56*	474	*46*	499	*36*
"	" II.....	436	*44*	420	*48*	410	*33*	410	*49*
"	" III....	429	*37*	426	*38*	406	*51*	409	*45*
"	" IV....	436	*47*	436	*46*	447	*53*	439	*60*
"	" V.....	480	*59*	482	*81*	459	*50*	468	*54*
"	**Average**.....	**441**	**48**	**452**	**54**	**439**	**47**	**453**	**49**
Subject	Series I......	410	*35*	407	*44*	417	*32*	416	*35*
Ww	" II.....	391	*37*	395	*33*	416	*35*	401	*29*
"	" III....	356	*37*	362	*42*	361	*34*	372	*22*
"	" IV....	364	*25*	364	*32*	381	*24*	360	*38*
"	**Average**.....	**380**	**34**	**382**	**38**	**394**	**31**	**387**	**31**
Subject	Series I......	552	*50*	543	*43*	550	*44*	548	*65*
Sz	" II.....	522	*58*	543	*63*	487	*45*	561	*62*
"	" III....	497	*39*	505	*43*	496	*55*	514	*41*
"	" IV....	430	*52*	424	*50*	470	*49*	471	*34*
"	" V.....	455	*53*	450	*43*	454	*59*	494	*45*
"	" VI....	569	*64*	606	*54*	531	*58*	585	*60*
"	" VII....	534	*52*	532	*47*	533	*54*	539	*42*
"	**Average**.....	**508**	**53**	**515**	**49**	**503**	**52**	**530**	**50**
				Summary					
Vs	Average.....	441	*48*	452	*54*	439	*47*	453	*49*
Ww	"	380	*34*	382	*38*	394	*31*	387	*31*
Sz	"	508	*53*	515	*49*	503	*52*	530	*50*

The results are given in Table XIV. Inspection of the summary of the table shows that the reaction time does not vary much with the absolute intensity. Both the average reaction times and the average mean variation remain strikingly near constant, in spite of the very great variation in absolute intensity. There is, indeed, some slight variation, but in this sort of measurements such very slight variation, when it is irregular in direction, is sufficiently taken account of by being designated as due to "accidental variation." Some significance, perhaps, attaches to the increase in reaction time with the weakest intensity which occurs in the case of subject Sz, though the difference between this reaction time and that obtained with the strongest intensity is less than one-half the average mean variation for either. And even with this subject the next to the weakest intensity gives a shorter reaction time than the strongest. We may conclude, therefore, that as long as the ratio of intensities remains the same the absolute intensity of the stimuli may vary widely without changing the reaction time. A variation in absolute intensity as great as that here used would produce a very marked variation in simple reaction time, nearly have doubled it, as I know from experiments with the same subjects reported in Chapter III. A variation in the absolute intensity of the stimuli, therefore, great enough to produce a very great variation in simple reaction time does not affect the time of a reaction such as described above. This conclusion does not eliminate the possibility of an increase in the reaction time with extremely weak intensities, intensities that are close to the limen, nor with somewhat greater intensities in the case of a very much smaller relative difference in intensity than the one used in obtaining the above results.

Now, if a decrease in the eye's sensitivity to intensity of light may be likened to a gray glass screen placed between the eye and the stimulus, as I have already argued, then the conclusions just drawn will hold also for variations in sensitivity. A gray glass held before two stimuli of different absolute intensity decreases the intensity of each by the same proportion. Such equal proportional decreases in intensity, however, have just been shown to have no effect on the time of reactions, such as the above, to a decrease in intensity,—at least within very wide limits of absolute intensity. We may consequently conclude that the time of reaction to a given change in intensity does not vary with variation, within wide limits, of the sensitivity of the eye to intensity. This conclusion could be foretold if we were sure Fechner's law or some similar law were valid. But while it is in keeping with Fechner's law, it does not demonstrate the same, for the reason, already pointed out, that it is quite possible that the time of a reaction such as I have used might not vary even though the apparent difference in the intensities of the stimuli, or the apparent change in intensity, underwent a slight variation.

With the idea of measuring attention in mind, I should draw the further conclusion that variations in the eye's sensitivity to intensity, within wide limits, would not produce any difference in the degree of attention present in a reaction based upon the recognition of a change from one intensity to another. Any method of measuring attention which makes use of such reactions, therefore, is not affected by even quite large variations in the sensitivity of the eye. It is true that I have not tried again the method used in Chapter III of applying a detraction test for each different absolute intensity; but since these different intensities all gave about the same reaction time this seemed quite unnecessary. In a case like this, *where all conditions but absolute intensity may be regarded as constant,* any change in the degree of attention, produced by change in the absolute intensity, would certainly show in the reaction time with the regularly repeated 2 sec. preparatory interval. This was shown to be true in the last chapter even in the case of simple reaction times, and the more complex form of reaction described in the present chapter is certainly as sensitive to changes in attention as the simple reaction. The preceding experiments, therefore, show how attention may be measured independently of the retina's sensitivity. The procedure need not differ from that used in Chapter III except that a given change in intensity would be used as the stimulus instead of a given absolute intensity, and a set of irregularly mixed intervals would be substituted for the long regularly repeated intervals, as the detractor of attention, so as to make the result independent of the subject's ability to estimate intervals.

Some experiments were next made with the same sort of reaction as described above, but the size of relative change in intensity was varied while the absolute intensity of one of the stimuli remained the same throughout. In these experiments a set of irregularly mixed preparatory intervals was used in each case as well as a regularly repeated 2 sec. interval. My object was primarily to test out again the method of using unfavorable intervals as a detracting condition, and the law already arrived at, that the absolute effect of such detracting conditions varies inversely with the degree of attention. It seemed probable that, since in the present instance it is a question of attention to a change in intensity, different degrees of attention would be brought about by varying the size of the change in intensity of the stimulus. This presumption was confirmed by the fact that such variation was found to produce variation both in the reaction time obtained with the regularly repeated 2 sec. interval and also in the absolute detraction effect produced by the use of irregularly mixed intervals. In the present instance three bars of light were used instead of two. The change in intensity might affect any of the three, and the subject had to call out "right," "left" or "middle," according to which of the three was observed to change in intensity. The object of using three bars instead of two was that it was thought that thereby an order could be used which would make it harder for the subject to anticipate in which bar the change was to occur. The order used was a chance order, determined by drawing gun wads with *r, l* or *m* marked on them, was different in every series and included the right, left and middle bars equally often. The use of three bars instead of two may account for the fact that incorrect answers were occasionally given by the subject, as none occurred in the previous experiments reported in this chapter, where only two bars were used. This difference in result in the two cases is, however, apparently due more to the fact that in the case where two bars of light were used, the change in intensity used was large, whereas in the case where three bars were used small changes in intensity, as well as large, were used. The incorrect answers occurred chiefly, though not exclusively, with the smaller changes in intensity.

The apparatus was the same as in the experiment already described in this chapter, except that the stereopticon slide had three oblong apertures instead of two, thus producing a stimulus consisting of three oblongs or bars of light. The change in intensity was produced, as before, by the dropping of a photographically developed glass strip, attached to the upper shutter of the tachistoscope. To obtain different amounts of change, slides developed to different shades of gray were used. I had a large array of such, and cut out strips which appeared to differ in shade about equally from each other. No measurements were made of the proportion of light cut off by the different strips, but there can be no doubt of the correctness of the order of the size of the changes in intensity produced by them. Five different changes were used, which are numbered from 1 to 5 in Table XV, 1 being the smallest change used and 5 the largest. With each size of change in intensity, first, 30 reactions were taken with a regularly repeated preparatory interval of 2 secs., and then 30 with a set of irregularly mixed preparatory intervals. This finished the work done at any one sitting. Two sittings were devoted to the same change in intensity before going on to the next size of change. The different sizes of change in intensity were used in the following order: Beginning with 1 and proceeding in regular order to 5, and then with 5 again and going back to 1. The total number of reactions for each subject with each size of change in intensity was, therefore, (2 × 30) × 2 × 2, or 240. The same set of irregular intervals was used as in the experiments reported in Chapter II, in connection with Table V, with the addition to that set of 25 of the following 5 : 12, 20, 8, 4 and 16 secs. The source of illumination throughout was a 250-watt Mazda tungsten lamp. The light stimuli were visible before the warning signal, a short ring of a muffled electric bell, and in fact were visible all the time except while the tachistoscope was being adjusted to bring the gray glass strip over a different bar of light.

I have already presented data on the effect of an irregular series of preparatory intervals and argued that the prolonging effect of such a series, upon reaction time, is due to the fact that the reactions occur with a lesser degree of attention. Additional evidence of a very decisive character that the reactions with irregular intervals occur with less adaptation of attention than the reactions with the regularly repeated 2 sec. interval was obtained in the course of the present experiment. This evidence consists in the fact, that when a very slight change in intensity was used, yet not so slightly but that it was promptly noticed if it occurred at the end of regularly repeated intervals of 2 secs. it was often *not noticed at all* with the totally irregular intervals. It has already been pointed out that the sudden decrease in the intensity which occurred in one of the stimuli was simply observed as a sort of a flicker, and that the observation of this flicker did not necessitate any comparison between the separate bars of light. With a very large decrease in intensity, this flicker is very easily noticeable, but with an extremely small change in intensity it is exceedingly difficult to notice. Yet, when the regularly repeated 2 sec. interval was used, even the smallest decrease used was practically always noticed,—in every case with one subject and in all but two cases wth the other subject. But with irregular intervals, when a very small change was used, the flicker effect produced by the change was very frequently unnoticed. Even with some of the larger changes it was occasionally unnoticed. This was determined, directly, by the subject's reports, and, indirectly, by observation of the reaction times. The subject noted on paper after each reaction, whether he had called "right," "left" or "middle," so that his responses could afterwards be checked up as right or wrong. He also noted any disturbance or anything unusual of any sort, and further made note each time that he did not observe the flicker effect. I found that when the flicker effect was not observed the reaction word was usually correct but the reaction time at least several hundred sigma longer, sometimes a whole second or two longer, and sometimes no reaction occurred at all. Both subjects gave the introspection that in case no flicker was observed and yet a reaction occurred, it was after a comparison of the brightness of the two lights. This comparison occurred either merely accidentally because the subject had not much else to do than to fixate the two lights, or else because he had a doubt as to whether one of them had flickered, and made a deliberate comparison of the brightness of the three bars in order to settle his doubt.

This frequent failure of the subject to notice a flicker with irregular preparatory intervals, when one was noticed every time with a regularly repeated 2 sec. interval, can only be explained on the ground that in the case of the irregularly mixed intervals there existed at the time of the flicker a lower degree of attention to the light stimuli. The failure to notice the flicker, notwithstanding the very short duration of the same, was certainly not due to winking. The subjects were both able to refrain from winking for periods much longer than 20 secs., without difficulty, as was determined by an actual test, and both felt sure they did not wink during the preparatory intervals. It is, therefore, highly improbable, even though some unobserved winks may have occurred, that these would have coincided with the change in intensity, in as many cases as 10 or 14 out of 30. Nor can the failure to observe the flicker be explained as due to inaccuracy in the fixation of the stimuli by the eye, in the case of the irregular intervals. It would be very hard to show that a slight inaccuracy in fixation of a fairly bright light would change the size of the differential limen. And, moreover, if the eyes did wander more with irregular intervals than with regular, in view of the results presented in Tables XVII and XVIII, Chapter V, it could hardly be because they were more fatigued, but merely because the attention wavered. We have here then a striking proof that the use of an irregularly mixed series of widely different preparatory intervals decreases the degree of adaptation of attention to the reaction process. Further, since the proportion of times the flicker was unnoticed decreases as the size of the change in intensity increases, we may conclude that the degree of attention to a change in intensity is conditioned by the amount of the change in intensity, just as the degree of attention to a simple stimulus is conditioned by its absolute intensity. This conclusion is identical with the assumption upon which the experiments of this chapter are founded.

The frequent occurrences of these cases in which with irregularly mixed intervals the flicker was not observed constitutes a certain defectiveness in the technique when the primary object of the experiment is borne in mind. This was to determine the effect of the irregular set of intervals, and to determine how this effect varied with the size of the change in intensity used. Since when a small change in intensity was used it was sometimes noticed, but frequently not, or at least not noticed directly, it becomes impossible to average the reaction times obtained in these cases, and thus the calculation of the effect of the irregular set of intervals in these cases is difficult and in an accurate quantitative way impossible. Another difficulty which arose consisted in the fact that often the subject reacted incorrectly, e.g., calling out "right" in place of "left."

Neither the times of incorrect reactions nor those in which the flicker effect was not observed are averaged with the others. The number of such reactions is noted in Table XV for each change in intensity. The average given is for all the remaining reaction times, the number thus averaged in the case of each degree of change in intensity being 30 (30 with regularly repeated, and 30 with irregularly mixed, intervals), minus the number of incorrect reactions and the reactions where no flicker was observed.

In column A of Table XV are given the times obtained with a regularly repeated 2 sec. interval and in column B are given the times with the set of totally irregular intervals. The numbers 1 to 5 occurring in the column headed Degree of Change in Int. refer to the amount of decrease in the intensity of the bar of light over which the gray glass slide dropped. The number 1 represents the smallest change in intensity and 5, the largest. It is evident that the averages given for the smaller changes in intensity are quite misleading, as many were thrown out. It seems certain that in those cases in which no flicker was observed

TABLE XV

The Effect, upon the Time of Reaction to a Change in Intensity of Stimulus, of Variation in the Degree of Change, both with a Regularly Repeated Preparatory Interval of 2 Secs. and with Irregularly Mixed Preparatory Intervals.

N, for each average, with each degree of change in intensity = 120. Total No. of reactions = 2,400.

	Degree of Change in Int.	A (R. T. 2 secs.)		B (R. T. Irreg.)		B−A	Flicker Unobserved		Incorrect Reaction Word	
Subject		Av.	M. V.	Av.	M. V.		2 secs.	Irreg.	2 secs.	Irreg.
Vs	No. 1	551	83	763	103	212	..	7	1	..
"	"	559	85	746	97	187	1	12	..	..
"	"	528	60	728	113	200	..	8	3	..
"	"	531	75	805	108	274	..	6	1	..
"	No. 2	464	62	662	89	198	..	6	2	..
"	"	483	46	667	65	184	..	4	..	1
"	"	494	49	703	77	209	1	4	..	..
"	"	436	31	621	56	185	..	8	1	2
"	No. 3	439	36	636	65	197	..	1	..	1
"	"	460	48	619	77	159	..	..	..	1
"	"	440	36	600	67	160	..	2	3	..
"	"	468	30	672	76	204	..	5	3	1
"	No. 4	465	40	558	52	93	..	1	2	3
"	"	468	38	619	62	151	..	..	1	4
"	"	455	35	663	67	208	..	..	..	3
"	"	424	34	612	71	188	..	..	..	1
"	No. 5	444	50	598	87	155	..	1	..	..
"	"	453	60	628	54	175	..	..	4	3
"	"	474	36	600	54	126	..	1	..	..
"	"	445	50	579	49	134	..	..	2	2
Subject	No. 1	564	70	791	81	227	..	4	..	1
Ww	"	492	57	676	58	184	..	4	..	..
"	"	477	46	652	50	175	..	6	..	..
"	"	514	55	721	108	207	..	9	..	1
"	No. 2	500	69	656	133	156	..	1	..	..
"	"	489	70	633	80	144	..	1	..	..
"	"	451	48	565	64	114	..	2	..	1
"	"	455	33	606	44	151	..	1	..	..
"	No. 3	439	44	564	65	125	..	..	..	..
"	"	431	43	579	72	148	1	..	..	..
"	"	460	35	574	46	114	..	1	..	1
"	"	486	34	588	40	102	..	..	..	..
"	No. 4	442	42	576	55	134	..	..	..	..
"	"	431	49	587	62	156	..	..	..	..
"	"	418	25	511	57	93	..	..	..	..
"	"	429	42	532	42	103	..	..	..	..
"	No. 5	415	41	497	92	82	..	..	1	..
"	"	421	41	530	33	109	..	..	..	..
"	"	430	36	540	54	110	..	..	..	..
"	"	431	50	534	48	103	..	..	..	..

Summary

Subject	No. 1	542	...	760	...	**218**	1	33	..	..
Vs	No. 2	469	...	663	...	**194**	1	23	..	..
"	No. 3	452	...	632	...	**180**	..	8	..	..
"	No. 4	453	...	613	...	**160**	..	1	..	..
"	No. 5	454	...	601	...	**147**	..	2	..	..
Subject	No. 1	512	...	710	...	**198**	..	23	..	..
Ww	No. 2	474	...	615	...	**141**	..	5	..	..
"	No. 3	454	...	576	...	**122**	1	1	..	..
"	No. 4	430	...	552	...	**122**	..	..	..	..
"	No. 5	424	...	525	...	**101**	..	..	..	..

the attention was the poorest, and therefore that the reaction times in these cases which were thrown out, were extremely long. The greatest number of "flicker unobserved" cases occurs with the two smallest changes in intensity, with irregular intervals. Consequently in interpreting the results, it must be remembered that the averages given for changes 1 and 2 in intensity with irregular intervals are misleadingly short. In the column headed B-A, is given in σ the absolute prolongation in reaction time produced by the use of the irregular set of intervals. As has been pointed out, the absolute prolongation so produced varies inversely with the degree of attention.

A study of the summary of the table shows that the reaction time with the regularly repeated 2 sec. interval manifests a tendency to increase with decrease in the size of the change in intensity. This tendency is not shown very uniformly, but is marked, especially in the case of the smaller changes in intensity. The reaction time with irregular intervals also increases with decrease in the size of the change in intensity, but in a much more marked manner, so that the difference between the time with regular and irregular intervals increases greatly as we pass from the largest change in intensity to the smallest. In other words, the absolute prolongation in reaction time produced by the irregular intervals increases as the size of the change in intensity decreases. This occurs in spite of the fact, pointed out in the preceding paragraph, that the reaction times with irregular intervals and the smaller changes in intensity are misleadingly small. When allowance is made for this latter fact, it is evident how enormously greater the prolonging effect of the irregular intervals is in the case of the smaller changes in intensity.

The fact of the increase in cases of "flicker unobserved" with decrease in the size of change in intensity, does of itself constitute a proof that the size of change is a condition of attention in this experiment. Consequently, the results obtained with each of the five sizes of change in intensity were obtained with different degrees of attention on the part of the subject, the best attention occuring in the case of size of change No. 5, and the worst, in the case of No. 1. Since the magnitude, B-A, varies inversely with the size of the change in intensity, the results evidently confirm the law already arrived at, that a given set of unfavorable preparatory intervals results in an absolute prolongation of reaction time varying inversely with the degree of attention involved in the reaction, or, more generally, the absolute effect of a given detractor of attention varies inversely with the degree of attention on which the detractor acts. As a matter of fact, however, it is so plausible from general considerations that the size of a change in intensity determines its noticeability and its clearness and is thus a condition of attention, that apart from the increase in cases of "flicker unobserved" it would be safe to assume that different degrees of attention prevailed as the size of the change in intensity was varied. And both the increase in the number of "flicker unobserved" cases and the lengthening of reaction time, produced in each case by the irregular intervals, are detraction effects resulting from the use of the set of irregular intervals as a detractor. Consequently, in both of these kinds of detraction effect, taken together or singly, we have confirmation of the law that the absolute detraction effect of a given detractor varies inversely with the degree of attention.

In the preceding discussion of the data presented in Tables XIV and XV the feature that has been most emphasized is that the reactions involved a recognition of change in intensity. A fact of considerable practical importance in the measurement of attention, however, is brought out when we remember that the reactions reported in the present chapter are "choice" reactions. The form of the reaction depends upon a certain feature of the stimulus by which the subject must be affected before he can react properly, so that, in spite of the fact that the subjects were instructed to react as quickly as possible, there was no danger of premature reactions. In my own work I have not felt that premature reactions were a source of difficulty. I have frequently met with them, but those so premature that the reaction had actually occurred before the stimulus would never affect the chronoscope at all, and only very seldom have I gotten reaction times so short that I could be sure that the reactions had been prematurely initiated. On the whole I believe that the reaction times that I obtained were distributed fairly symmetrically, as in the sample distribution presented in Table II, Chapter II. I have not actually plotted the distribution of all the reaction times I have taken, but the very short reactions that I have obtained I believe to be fully counterbalanced by the very long ones. Yet it may very well be that with some subjects premature reactions would prove troublesome. Indeed it may be that the results here reported, especially those in Chapter V on practice, have been influenced to some extent, without my detecting it, either by a slight tendency to premature reactions or by carefulness on the part of the subject to avoid premature reactions. Should it be found that a tendency to premature reactions is sometimes troublesome in the case of the simple reactions with a regularly repeated 2 sec. interval, the results of the present chapter show that this difficulty may be met by using choice reactions, as in the experiment reported in Table XIV.

The reaction time of choice reactions in influenced fully as much by irregularity in the preparatory intervals as is simple reaction time. The same law of detraction holds for both sorts of reactions, and the procedure for measuring degree of attention would be the same for both forms of reaction. Further, it seems exceedingly probable that a true discrimination reaction could also be satisfactorily employed. A reaction to either an increase or a decrease in brightness, the reaction word being "more" in one case and "less" in the other, would not only be safe from premature reactions but would preserve the advantage inherent in the use of a change in brightness as a stimulus, namely, independence, within wide limits, of variation in retinal sensitivity.

SUMMARY

The time of a reaction to a change in intensity varies with the size of the change in intensity. This is shown in Table XV by the increase in reaction time, with either regular or irregular intervals, with decrease in the size of the change in intensity.

The time of a reaction to a change in intensity remains very nearly constant for a very wide range of absolute intensities of stimulus, providing the relative change in intensity is kept constant. From this, it seems probable, by analogy, that the reaction time to a given change in intensity would remain very nearly constant throughout a wide range of change in retinal sensitivity to intensity, other factors than retinal sensitivity remaining constant. Such a constancy in the reaction time would indicate a constancy in the degree of attention, conditioned by the given change in intensity, in spite of variations in retinal sensitivity to intensity. It follows, therefore, that by the use of a reaction time to a change in intensity, *attention can be measured independently of wide variations in retinal sensitivity.* This is of considerable importance in the comparison of different individuals. The method, using such a reaction, would be to determine the reaction time with a regularly repeated preparatory interval of 2 secs., and then to determine the absolute prolongation in this time produced by the use of a given set of totally irregular preparatory intervals, keeping the other conditions constant, and to rank the attention as the reciprocal of the prolongation obtained.

A smaller degree of adaptation of attention to the reaction exists in the case of reactions following irregularly mixed preparatory intervals than those following regularly repeated preparatory intervals of 2 secs. This conclusion was arrived at in Chapter II as a result of a study of the literature on the effect of the preparatory interval on reaction time, and is a basic proposition of the present monograph. Table XV contains an experimental confirmation of this proposition in the fact that it shows that slight changes in intensity were very frequently not noticed with irregular preparatory intervals while they were always, or practically always, noticed with regularly repeated intervals.

The attention involved in a reaction to a change in intensity decreases with decrease in the degree of change in intensity.

Table XV shows that both absolute prolongation in reaction time and increased frequency of the cases in which the change in intensity was unnoticed resulted from the use of irregular preparatory intervals in the case of each degree of change in intensity; and that both these detraction effects of the irregular intervals increased markedly as the degree of the change in intensity decreased, or, consequently, as the degree of attention decreased. So, finally, we may conclude again, as in Chapter III, but from different experimental data, that the absolute detraction effect of a given detractor varies inversely with the degree of attention against which the detractor acts.

Irregularly mixed preparatory intervals constitute fully as effective a detractor of attention in the case of choice reactions as in the case of simple reactions. Consequently, if it should be found that premature reactions were a source of error in the measurement of attention by the procedure here proposed, a choice reaction or a true discrimination reaction could be used and thus the difficulty avoided. A change in brightness as reaction stimulus, which might be sometimes an increase and sometimes a decrease, would not only be safe against premature reactions but would also preserve the advantage that is inherent in the use of a change in intensity as a reaction stimulus, namely, independence, within wide limits, of variation in retinal sensitivity to intensity.

CHAPTER V

Illustrative Applications

(a) practice

A serious drawback to almost all, if not all, of the mental tests now in use is that the result obtained varies with the degree of practice which the subject has had in the activities or functions measured by the test. The result of one or a few applications of the test, therefore, serves as a measure of the trait only at a particular stage of practice,—it measures neither the original ability nor the final ability. Even in adults, all of whom have already had a great deal of practice in the trait or complex of traits tested, marked improvement may occur with even moderate practice, and this improvement varies in amount with different individuals and with different traits. Practice effects are not only invariably found in the case of simple mental tests, but also in the case of our most elaborate and scientific mental measurements. And reaction time itself usually shows a marked shortening as the result of practice. The problem of practice is very evidently, therefore, one of great importance throughout the whole field of mental tests and measurements, and in general constitutes a serious obstacle. Many of the conclusions that have been arrived at from the use of mental tests and measurement are valid, but many of them would be seriously modified by correction for the practice error. Without this correction it is hard to see how a satisfactory comparison can be made between different individuals, between different traits in the same individual, or of the effect of various conditions acting at different times on the same mental trait in a given individual. There are three simple ways, theoretically, of obviating the difficulty. One is to make the measurement before practice has begun, one to make the measurement always at the same stage of practice, and one to make it at the terminal stage, that is, after such long continued practice that no further improvement is noticeable. But all three of these methods are usually more or less impracticable and it is seldom that any one of them has been rigorously applied.

Our knowledge of what "practice" means is in its infancy. While we may define it as the improvement in a function resulting from the exercise of that function, it must be admitted that very frequently indeed we assume altogether without warrant that we are measuring the same functions at the beginning and end of our practice series, whereas it is quite likely that the quality or type of function changes with practice, in which case the measured increase in efficiency does not so much represent an improvement in the given psychophysical processes as the adoption of different ones. To understand practice, we must not be content to note merely the improvement in the results of the psychophysical function. We must determine the factors that work together to produce the results, and then determine whether there has been improvement in these factors, and in which of them, or whether on the other hand merely a similar but better result is being produced by different factors. For example, because attention is involved in a certain psychophysical function. and the results show practice effects, we cannot complacently explain the practice effect as due to improvement in the attention as the result of the practice. We could as correctly speak of the practice effect in electro-motive force if we observed that the output of some electrical plant improved with the time it had been in operation, even though we were not sure but that the manager had been constantly introducing different machinery and altering his method. At present the effects of practice have not been studied sufficiently analytically to enable one to say, in most cases at any rate, to what practice is due. We can easily determine the change in the output, but we know very little about the accompanying changes that occur in the "plant." We do not at present know what psychophysical functions improve with practice, not to raise the question in what the improvement consists.

Attention cannot be measured, of course, without its being exercised, so that each time the subject's attention is measured the subject has practice in attention. I consequently undertook the investigation of the effect of practice on the degree of attention. My problem was merely whether I should get the same result time after time throughout a long series of measurements. It is true that simple reaction time under any given set of conditions usually shows a very decided practice effect. But, as usual, we do not know to what the practice effect is due. If reaction time decreases with practice, because of increase in the degree of attention, I should get a practice effect in my measurements of the degree of attention. If, on the other hand, it is true that a high degree of attention is present only in the initial stages of learning a habit, and that as the reaction is repeated it tends to become reflex and consequently to involve a lower degree of attention, my measurement of attention, if valid, should show the opposite of a practice effect. The actual data obtained tend to show that neither a practice effect nor the opposite is the result. The degree of attention was not found to show any general tendency either to increase or decrease with repetition of the measurements.

The methods used in the present experiments on practice are similar to the method described in the last chapter, though considerable simplification of technique was introduced. The reactions were motor or abbreviated reactions to a change in intensity. The conclusion was reached in the last chapter that the use of a change in intensity as the reaction stimulus renders possible a fairer comparison of individuals. A comparison of different individuals is not particularly important in the present instance, but the fact that a reaction to a large supraliminal change in intensity is independent within wide limits of the absolute intensity or of the sensitivity of the sense-organ, renders it unnecessary to use a dark-room, or to allow any particular time for adaptation of the retina to the prevailing illumination, or to work at all times under the same illumination. Consequently in the present work all these factors were allowed to vary somewhat and no particular attention paid to them. In place of two or three lights or bars of light, only one was used. I found in the work with several bars of light, that when one changed in brightness, the change was noticed directly, and that either no comparison at all occurred between the brightness of the bar which changed and the other bars, or else such a comparison was incidental. Moreover, in the present experiment the change in brightness was produced in a different manner. Instead of the elaborate tachistoscopic apparatus for the exposure of the light used in the experiments described in Chapters III and IV, the single light used was arranged so as to be visible at all times, and its increase in intensity was produced merely by increasing the current in the electrical circuit which caused the illumination. This was accomplished by throwing into the circuit a parallel shunt, using a double key, so that the shunt was thrown in synchronously with the make of the chronoscope circuit. Another simplification was the return to an ordinary reaction key, Scripture's so-called "noisless key," in place of the Cattell voice-key, and the use of an ordinary break (upward) reaction movement The click of an electric sound hammer was used in place of the ring of an electric bell for the warning signal. To estimate the intervals the experimenter made use of a noiseless pendulum in place of a metronome. This last change was due to the fact that the adjoining subject's room used in this part of the work was by no means sound proof, being separated from the experimenter's room by an ordinary tile wall and wooden door. The subject could even hear the starting of the chronoscope. The chronoscope was not stopped between successive reactions, but allowed each time to run until the driving weight reached the bottom, or nearly so.

The light stimulus was an all-frosted 25-watt Mazda lamp placed behind an aperture 4 cms. square in a dark box. The aperture was covered by ground glass. The intensity of this light was not measured, though the conditions could be fairly accurately reproduced from the description below. Before the increase in brightness constituting the reaction stimulus, the light was bright enough to be visible easily, what might be called a moderately dim light. The change in intensity, likewise not measured, was very great, so that the light looked nearly twice as bright after the change as before. The current used was a direct current of 110 volts, supplied by the University power plant. With the arrangement used a slight or even considerable variation in intensity of the circuit supplied would not appreciably affect the noticeability of the relative change in intensity, which is the only thing it was necessary to keep constant. The 25-watt lamp was placed in this 110-volt circuit in series with a 40-watt Mazda 110-volt lamp, with no other resistance except a number of feet of copper wire necessary for the connections. The increase in brightness was produced by throwing into the circuit a shunt containing a second 40-watt Mazda, in parallel with the other 40-watt Mazda. This produced an extremely sudden increase in brightness.

Of course, this increase in brightness, while apparently instantaneous, required really a few thousandths of a second. The method would be unpermissible if it were a matter of obtaining absolute reaction times. But my only object was to measure the absolute prolongation in reaction time resulting from the use of irregular intervals. The time required for the change in brightness to occur is a constant when sufficient interval is always allowed the light each time to return to its original brightness before each increase. This interval, which to the eye appeared to be almost nothing, was certainly many times less than the smallest interval between any two reactions with either the regularly repeated intervals of 2 secs. or the irregular intervals, and thus in both cases the time required for the change in intensity constituted a constant error which would not affect the absolute difference in reaction time. No attempt, consequently, was necessary to correct for this error.

From the description of the method of producing the change in intensity which constituted the reaction stimulus, it will be seen that the entire apparatus was very simple. A standard chronoscope and fall-hammer circuit, three lamps and a dark box is all that was required. The manipulation on the part of the experimenter was equally simple. At the same time the apparatus seems entirely satisfactory for its purpose.

Unfortunately for the sake of uniformity, the results I have obtained on the effect of practice were not all taken with the same apparatus. In addition to the apparatus just described, two sets of practice series, those with subjects Vs and Sz, presented in Table XVIII, were taken with the apparatus described in Chapter I in connection with Table I, where a sound stimulus was used.

Both methods are equally good, for the study of practice effects, but the one described in this chapter is better for purposes in general, since it permits a comparison of individuals apart from sensory sensitivity. The present study extended over several years and each part indicated some improvement in method, which was usually adopted in the following parts. And some study of practice as well as some preliminary applications were made at various stages of the work, thus accounting for the use of more than one method in the experiments described in this chapter.

In all cases the procedure was essentially the same. The subjects served only one hour a day, for a number of successive days, Sundays omitted. Each day, reactions were first taken with a series of regularly repeated 2 sec. preparatory intervals, and then with a series of irregularly mixed preparatory intervals varying fom 4 to 20 secs. In the case of the two practiced subjects whose results are given in Table XVI, 30 reactions were taken with the regularly repeated 2 sec. intervals, and then 30 with the irregularly mixed intervals; and this constituted the entire procedure for one day. In the case of the two subjects included in Table XIX, the procedure was the same as that just mentioned, except that 50 reactions instead of 30 were taken with each of the two kinds of preparatory intervals, i.e., the regularly repeated 2 sec. intervals and the irregularly mixed intervals.[1] In the case of the subjects included in Tables XVII and XVIII, the procedure was as follows: first, 30 reactions were taken with the regularly repeated 2 sec. intervals, and then 30 with the irregularly mixed intervals; then, after a few minutes intermission, during which the subject rested and the experimenter tested the accuracy of the chronoscope by means of the Wundt's fall-hammer, 30 more reactions were taken with the regularly repeated 2 sec. intervals and then, lastly, 30 more with the irregularly mixed intervals. In these cases the average reaction time for the 2 sets of regularly repeated 2 sec. intervals is subtracted from the average reaction for the 2 sets of irregularly mixed intervals, and the difference considered as constituting a single measurement of attention. In all four tables the average (Av.) and the mean variation (M.V.) of each set of intervals used on each day is given in σ. In Tables XVII and XVIII, there are given, in addition, the average for all of the regularly repeated 2 sec. intervals (2 secs. Av.) and for all of the irregularly mixed intervals (Irreg. Av.) used during one day's sitting. In the last column at the right, in all the tables, headed 1/A, is given the absolute difference between the average reaction time for all the 2 sec. intervals and the average for all the irregular intervals, used at one sitting. Since the time with the irregular intervals is always greater, this column gives the absolute prolongation produced by the irregular intervals, or, in other words, the detraction effect of the irregular intervals. This is the quantity which it has been shown in the preceding pages varies inversely as the degree of attention, or directly with 1/A, in which A represents the degree of attention. In the Tables XVI-XIX the results of the measurements are presented in the order in which the measurements were made, the order being indicated by the column headed No.

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[1] The set of 30 irregularly mixed intervals used in obtaining the results presented in Tables XVI, XVII and XVIII was the same as used in connection with Table XV of the last chapter, namely: 4, 16, 20, 4, 8, 20, 12, 16, 8, 4, 12, 4, 20, 8, 12, 4, 16, 20, 8, 16, 12, 12, 8, 16, 20, 12, 20, 8, 4, and 16 secs. The set of 50 irregularly mixed intervals used in connection with Table XIX was the same as the preceding set plus the first twenty of that set.

In all, eight subjects were used. Six of these, subjects Lm, Sh, Vs, Sz, Ll and St, were absolutely unpracticed subjects. Since practice effects are invariably found to be more marked at first than later,[2] it would seem that 10 or 12 hours of work distributed over as many days should be sufficient to determine whether or not such an effect were present. Two subjects, Mn and Ww, were both highly practiced in nearly all kinds of reaction time work. The results with these subjects are given to determine what degree of constancy can be expected in the results of the measurement of attention in the case of trained laboratory subjects. This is important, for even though a considerable degree of practice effect might invalidate the method for widespread use in the measurement of different individuals, it would by no means invalidate it for research work and other purposes in the case of practiced reagents. The results with these subjects are also interesting in comparison with the results on unpracticed subjects.

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[2] "Finally, if irregularity is discarded and all curves smoothed out, only those facts conforming to the 'law of the practice curve' are represented, namely, that a person improves in any work most rapidly at first and makes little and slow improvement after reaching a certain degree of ability."

Table XVI

The Variation in a Series of Measurements of Attention. Practiced Subjects. Visual Reaction Time Method.

N, for each average = 30. Total No. of reactions = 840.

		2 secs.		Irreg.		
Subj.	No.	Av.	*M.V.*	Av.	*M.V.*	1/A
Mn	1	159	*40*	235	*16*	66
"	2	172	*17*	235	*15*	63
"	3	178	*17*	260	*21*	82
"	4	171	*24*	227	*19*	56
"	5	165	*17*	233	*20*	68
"	6	173	*33*	233	*18*	60
"	7	163	*24*	241	*26*	78
"	8	158	*20*	224	*20*	66
"	9	170	*21*	237	*9*	67
Ww	1	166	*10*	248	*13*	82
"	2	175	*19*	256	*19*	81
"	3	167	*20*	255	*25*	88
"	4	166	*15*	251	*10*	85
"	5	165	*10*	245	*9*	80

The point of main interest in Tables XVI-XIX is whether the quantity 1/A, that is, the absolute prolongation produced by the irregular intervals, varies with repetition of its measurement.

Whitley, *An Empirical Study of Certain Tests for Individual Differences, Archives of Psychology,* 19, 1911, 136.

To determine this point, it is necessary to study somewhat in detail the results obtained with each subject. To take up first the two subjects Mn and Ww, who had had a great deal of previous practice in reaction time work of various kinds, it may be said that there is no sign of either a general practice effect

Table XVII

The Variation in a Series of Measurements of Atttention. Unpracticed Subjects. Visual Reaction Time Method.

N, for each average of each series = 30. Total No. of reactions = 4320.

Subj.	No.	1st Series 2 Secs.		1st Series Irreg.		2d Series 2 Secs.		2d Series Irreg.		2 Secs.	Ir-reg.	1/A
		Av.	*M.V.*	Av.	*M.V.*	Av.	*M.V.*	Av.	*M.V.*	Av.	Av.	
Lm	1	230	35	309	23	248	22	348	39	239	329	90
"	2	206	13	321	39	251	39	360	52	229	341	112
"	3	203	35	302	27	256	28	329	25	230	316	86
"	4	213	22	302	39	236	11	335	49	225	319	94
"	5	220	16	312	30	234	34	358	77	227	335	108
"	6	222	18	316	27	251	18	338	37	237	327	90
"	7	202	19	301	35	245	20	327	33	224	314	90
"	8	216	11	319	30	231	20	329	41	224	324	100
"	9	194	32	331	43	256	21	329	37	225	330	105
"	10	230	18	335	37	227	13	335	35	229	335	106
"	11	191	17	277	40	211	28	316	38	201	297	96
"	12	211	18	306	29	232	9	326	43	222	316	94
"	13	214	14	313	37	218	11	316	29	216	315	99
"	14	197	14	306	29	228	15	321	29	213	314	101
"	15	209	19	310	32	216	24	311	27	213	311	98
"	16	216	20	305	23	205	27	311	35	215	312	97
"	17	183	17	306	26	210	22	319	32	201	317	116
"	18	196	10	318	30	198	17	314	25	201	320	119
"	19	183	22	315	37	200	37	300	34	196	312	116
"	20	193	9	312	20	215	18	314	24	208	317	109
"	21	192	14	312	35	221	14	305	39	211	313	102
"	22	190	23	295	29	207	19	300	32	203	302	99
"	23	208	15	301	21	232	16	341	40	224	325	101
"	Av.	205	19	309	31	227	21	325	37	216	317	101
Sh	1	213	45	326	39	252	33	335	37	233	331	98
"	2	201	18	296	27	217	17	304	37	209	300	91
"	3	203	16	304	39	205	23	303	35	204	304	100
"	4	185	12	270	28	195	21	290	33	190	280	90
"	5	180	15	279	29	189	29	286	28	185	283	98
"	6	177	25	283	33	210	25	273	23	194	278	84
"	7	169	29	275	26	191	27	288	22	180	282	102
"	8	200	11	262	21	218	30	251	36	209	257	48
"	9	195	21	257	18	184	24	264	34	190	261	71
"	10	194	11	280	32	187	18	276	16	191	278	87
"	11	173	22	275	13	192	10	288	29	183	282	99
"	12	220	23	274	29	209	12	281	26	215	278	63
"	13	210	10	268	16	218	19	280	30	209	274	65
"	Av.	193	20	281	27	205	22	286	30	199	284	85

or the reverse. The results are presented in Table XVI. Improvement in attention would be indicated by decrease in the size of 1/A in successive measurements, whereas a lessening of the degree of attention would be represented by an increase in the size of 1/A. As a matter of fact no general tendency to increase or decrease is observable. The variations are altogether irregular in direction, and most of them of little reliability in view of the size of the mean variations. With subjects like these, evidently there would be no difficulty in using the method for accurate quantitative study of the laws and conditions of attention. With subject Mn, the absolute mean variation for nine consecutive measurements of 1/A is 5σ, and relative 7 per cent, and with subject Ww, for five consecutive measurements, the absolute mean variation is 3σ, the relative, 4 per cent.

The results with subjects who were complete novices at the

Table XVIII

The Variation in a Series of Measurements of Attention.
Unpracticed Subjects. Auditory Reaction Time Method.

N, for each average of each series = 30. Total No. of reactions = 2400.

Subj.	No.	1st Series 2 Secs. Av.	1st Series 2 Secs. *M.V.*	1st Series Irreg. Av.	1st Series Irreg. *M.V.*	2d Series 2 Secs. Av.	2d Series 2 Secs. *M.V.*	2d Series Irreg. Av.	2d Series Irreg. *M.V.*	2 Secs. Av.	Irreg. Av.	1/A
Vs	1	142	*17*	199	*32*	117	*15*	211	*44*	130	205	**75**
"	2	122	*14*	203	*24*	128	*12*	213	*31*	125	208	**83**
"	3	125	*13*	193	*27*	129	*17*	199	*19*	127	196	**69**
"	4	139	*13*	209	*33*	130	*17*	191	*27*	135	200	**65**
"	5	115	*12*	184	*31*	106	*13*	195	*38*	111	190	**79**
"	6	117	*13*	210	*39*	115	*23*	209	*29*	116	210	**94**
"	7	110	*17*	200	*43*	115	*11*	202	*31*	113	201	**88**
"	8	124	*12*	196	*25*	135	*12*	204	*26*	130	200	**70**
"	9	131	*17*	195	*24*	129	*18*	209	*25*	130	202	**72**
"	10	134	*12*	210	*22*	138	*16*	207	*27*	136	209	**73**
"	Av.	126	*14*	200	*30*	125	*15*	204	*30*	126	202	**76**
Sz	1	153	*20*	256	*38*	138	*31*	229	*38*	146	243	**97**
"	2	137	*23*	219	*37*	124	*21*	204	*32*	131	212	**81**
"	3	140	*14*	224	*34*	129	*17*	220	*34*	135	222	**87**
"	4	156	*22*	236	*26*	164	*15*	243	*22*	160	240	**82**
"	5	136	*18*	211	*26*	152	*23*	224	*27*	144	218	**74**
"	6	143	*15*	213	*23*	141	*16*	223	*37*	142	218	**76**
"	7	137	*12*	207	*24*	142	*15*	214	*30*	140	211	**71**
"	8	162	*13*	202	*27*	163	*14*	213	*26*	163	208	**45**
"	9	155	*14*	214	*17*	155	*18*	228	*28*	155	221	**66**
"	10	157	*13*	205	*23*	158	*17*	214	*29*	158	210	**62**
"	Av.	148	*16*	219	*28*	147	*19*	221	*30*	148	220	**72**

beginning of the work, presented in Tables XVII-XIX, show certain interesting peculiarities, but in no case, I think, a true practice effect. Subject Lm (Table XVII) who was given 23 tests, the greatest amount of practice given any of the subjects, certainly shows no practice effect, but if anything, the opposite of a practice effect. That is to say, no tendency is evident on the part of the magnitude 1/A to decrease as the number of times it has been measured increases. The general tendency is quite definitely for 1/A to remain the same throughout the series of measurements, or else to very slightly increase. As in the case of all this work on practice, every effort was made to impress it upon the subject that all reactions, both those with the regularly repeated 2 sec. intervals and those with the irregularly mixed

Table XIX

The Variation in a Series of Measurements of Attention.
Unpracticed Subjects. Visual Reaction Time Method.

N, for each average = 50. Total No. of reactions = 2600.

Subj.	No.	2 Secs. Av.	2 Secs. *M.V.*	Irreg. Av.	Irreg. *M.V.*	1/A
Ll	1	247	*38*	305	*38*	**58**
"	2	189	*23*	277	*33*	**88**
"	3	193	*25*	255	*31*	**62**
"	4	194	*26*	252	*27*	**58**
"	5	188	*14*	250	*25*	**62**
"	6	189	*21*	250	*23*	**61**
"	7	203	*24*	269	*28*	**66**
"	8	181	*10*	243	*30*	**62**
"	9	176	*13*	247	*19*	**71**
"	10	197	*19*	251	*21*	**54**
"	11	181	*23*	240	*20*	**59**
"	12	225	*23*	291	*19*	**66**
"	13	216	*14*	288	*15*	**72**
"	14	204	*38*	279	*22*	**75**
"	15	195	*16*	261	*18*	**66**
"	16	217	*21*	295	*25*	**78**
St	1	191	*22*	253	*42*	**62**
"	2	187	*12*	232	*25*	**45**
"	3	179	*24*	245	*28*	**66**
"	4	190	*24*	241	*28*	**51**
"	5	229	*23*	287	*24*	**58**
"	6	202	*36*	281	*22*	**79**
"	7	220	*20*	280	*22*	**60**
"	8	211	*27*	273	*28*	**62**
"	9	207	*28*	276	*25*	**69**
"	10	203	*30*	275	*18*	**72**

intervals, were to be made as quick as possible. The most prominent feature of the results with subject Lm is the variation in the magnitude of 1/A from day to day, rather than any general tendency on the part of this magnitude to regularly increase or decrease. Yet there is nothing astonishing in these variations from day to day, and it is not at all improbable that they were entirely paralleled by variations from day to day in the degree of the subject's attention. I was unable to obtain any information from the subject, who was a young woman of 28, which seemed to account for the variations from day to day, though there was considerable variation in the amount of sleep per night during this period, and in the amount of work she did before she came into the laboratory at 5.00 P. M. for the test. The mean variation in 1/A, however, for the entire 23 measurements was only 7 per cent, with an average of 101 σ. The simple reaction times, whether for the 2 sec. intervals or the irregular intervals, show a definite practice effect, though this effect is neither very great nor regular. The average, for the first 5 days, of all the reactions with the regularly repeated 2 sec. intervals is 230 σ, for the last 5, 208 σ; while the average reaction time with totally irregular intervals, for the first 5 days is 328 σ and for the last 5, 314 σ. The average of 1/A for the 1st 5 measurements is 98 σ and for the last 5, 105 σ.

The results with subject Sh (Table XVII) are less clear cut. Subject Sh certainly shows no practice effect, as regards the degree of attention, during the first 7 days; that is, there is certainly no general tendency on the part of the magnitude 1/A to decrease during this period. Since practice effects are most marked at the very earliest stages of practice, we should expect them to become noticeable, if present at all, before the end of the seventh day, with practice an hour per day. Indeed, in the case of the simple reaction times, both with the regularly repeated 2 sec. intervals and the irregularly mixed intervals, a very marked practice effect is noticeable. In spite of this practice effect in the simple reaction times during the first 7 days, there is no sign of a decrease in the magnitude 1/A. And yet on the eighth day, 1/A decreases very suddenly. From this point on, no further decrease in 1/A occurs, but on the other hand, a regular and marked increase, until the twelfth day, when a second drop in the size of 1/A occurs. It seems to me improbable that these two sudden decreases in the size of 1/A correspond to real increases in the degree of attention. Consequently, whatever the explanation of these sudden decreases in 1/A may be, I must recognize that there is here involved some serious source of error in my method. I shall attempt to show, however, that this error is not the result of any practice effect in the degree of attention, but due to the detrimental effect upon the favorableness of the 2 sec. intervals exerted by the long irregularly mixed intervals when the use of the latter is frequently alternated with the use of the regularly repeated 2 sec. intervals. I believe this error can be entirely avoided or at least rendered of negligible importance by a slight modification of the method, which I shall speak of in connection with Table XIX.

A study of the results for subject Sh will show that the two sudden drops in the value of 1/A correspond to two sudden increases in the reaction time with the 2 sec. preparatory intervals. The reaction time for the 2 sec. intervals, as the result of the first 7 days practice, decreases in a fairly regular manner from 233 σ to 180 σ. The reaction time for the irregular intervals shows a similar decrease. But on the eighth day, while the reaction time with the irregular intervals continues to decrease, a sudden slump occurs in the reaction time with the 2 sec. intervals, the latter jumping from 180 σ to 209 σ. The reaction time with the 2 sec. intervals then again improves, reaching 183 σ on the eleventh day, only to jump on the following day up to 215 σ, a time longer than any obtained after the very first day. This peculiar behavior on the part of the reaction time with the 2 sec. intervals fully accounts for the irregularity in the value of 1/A. If the cause of the decrease in the value of 1/A were an increase in degree of attention resulting from practice, it would be incredible that just at the points where the decrease in 1/A was strikingly apparent, a marked increase would be found in the simple reaction time with the 2 sec. preparatory intervals.

Before taking up the explanation of the results obtained with subject Sh, I wish to mention the results obtained with subject Sz (Table XVIII), as the results with the latter subject closely resemble those with subject Sh. In the case of Sz there are two marked increases in the reaction time with the 2 sec. intervals. One of these increases occurs on the fourth day and the other on the eighth day. On the fourth day the reaction time with the 2 sec. intervals rose to 160 σ, which is 14 σ longer than the reaction time for the first day and 25 σ longer than the reaction time for the preceding day. On the eighth day the reaction time for the 2 sec. intervals was 163 σ, which is 19 σ longer than that for the first day and 23 σ longer than that for the preceding day. No marked change occurs in the reaction time with the irregular preparatory intervals. The decrease in the size of 1/A, therefore, is clearly due to the fact that the reaction time with the 2 sec. intervals increased with practice instead of showing the normal decrease. In neither subject Sh nor subject Sz, therefore, can a practice effect in the degree of attention be noted. It is true that 1/A decreases and that I have claimed that 1/A measures the degree of attention. But an analysis of the results shows that the values of 1/A have been influenced by some factor which constitutes a source of error. The effect of this source of error has been just the opposite of a practice effect in that it consists in a prolongation of the 2 sec. reaction time.

In view of the results I have presented in Chapter II on the effect of the order in which preparatory intervals of different length are used, I think there can be little doubt that the lengthening in the reaction time with the 2 sec. intervals, noted in the case of subjects Sh and Sz, is due to the result of adaptation to the longer irregularly mixed intervals. As I have already explained, in the case of the two subjects in question, each day's procedure was, first to take 30 reactions with a preparatory interval of 2 secs., and then, 30 reactions with a series of irregularly mixed intervals varying from 4 to 20 secs. in length; and then, after a few minutes pause, to take 30 more reactions with a 2 sec. interval and lastly, 30 with the series of irregularly mixed intervals. Thus it will be seen that, while the 2 sec. interval was repeated 30 times in succession, this whole series of repetitions was alternated with the use of much longer irregularly mixed intervals. I have shown in Chapter II, Tables V-VII, that the natural favorableness of the short preparatory intervals is largely destroyed when short intervals are preceded by longer ones. A certain adaptation to the long intervals occurs, and this adaptation to the long intervals has the result that the subject is unprepared for a stimulus which comes at the end of a short interval; so that in a series of irregularly mixed intervals of various length, it is extremely likely that the shortest interval which occurs in the series will give the longest reaction time, in spite of the fact that this shortest interval when used by itself would give shorter reaction times than the longer intervals.

I have pointed out, further, in Chapter II, that adaptation to short intervals has a much less detrimental effect upon reaction time with long intervals than does adaptation to long intervals upon the reaction time to short intervals. Now it seems to me that the peculiar trend of the reaction times with the 2 sec. intervals, which I have already noted in the case of subjects Sh and Sz, is fully explained by this phenomenon of temporal adaptation. Adaptation to the series of irregularly mixed intervals carried over to the regularly repeated 2 sec. intervals and rendered the latter less favorable than they were at the beginning of the experiment. It might be thought that using the 2 sec. interval 30 times in succession would obviate the effect of adaptation to the longer irregularly mixed intervals. But it is quite possible that daily experience with the two sorts of intervals had the effect of producing a more or less fixed attitude which prevented the best possible adaptation to the regularly repeated 2 sec. intervals. In working with irregularly mixed intervals such as here used, there is little adaptation of attention to any particular interval, as was shown in Chapter II; and I think it quite probable that the subjects Sh and Sz formed a habit as the result of working with these intervals which they could not throw off when they returned to the 2 sec. intervals.

As regards the remaining three subjects, subject Vs, Table

XVIII, and Subjects Ll and St, Table XIX, it may be said that there is absolutely no trace of a practice effect in the values of 1/A. With subject Vs, Table XVIII, the average value of 1/A for the first 3 of the series of 10 measurements is 74 σ and for the last 3, 75 σ. The average value of 1/A for the entire series of 10 measurements is 76 σ with a mean variation of 7 σ, or slightly less than 10 per cent. In the case of subject Ll, Table XIX, who was given a series of 16 measurements, the average of the first 3 values of 1/A is 69 σ and of the last 3, 73 σ, while the average of the entire series of 16 measurements is 66 σ with a mean variation of 7σ, or 11 per cent. In view of the irregularity in the size of 1/A from day to day, it can hardly be said that there is any reliable general tendency for 1/A either to increase or decrease. In the case of subject St, Table XIX, there is a slight tendency for 1/A to increase with repetition of its measurement, that is, for the opposite of a practice effect. This increase in the size of 1/A is not so marked, however, but that it may reasonably be explained as due to a wearing off of interest, or a decline in zeal, with the progress of the work. The average of 1/A for the first half of the series of 10 measurements is 56 σ and for the second half is 68 σ, while the average for the entire series is 62 σ with a mean variation of 7 σ, or 11 per cent.

It will be observed that the sudden decreases in the value of 1/A which occurred after a certain number of repetitions of its measurement in the case of subjects Sh and Sz did not occur in the case of any of the four subjects whose results are presented in Tables XVI and XIX. It will be observed, also, that the procedure used in obtaining the results presented in these last mentioned tables did not involve the use of the 2 sec. intervals directly after the use of the irregularly mixed long intervals as did the procedure used with subjects Sh and Sz. I am inclined to believe, therefore, that the error of temporal adaptation already alluded to can be avoided merely by always avoiding the use of 2 sec. intervals soon after the use of the irregular intervals. The 2 sec. intervals ought to be used first, and then the irregularly mixed intervals, and then the test should end, not to be repeated for at least one day. I am inclined to believe further that the error of temporal adaptation will be still more surely avoided by taking not more than 30 reactions with each of the two sorts of preparatory intervals, i.e., the 2 sec. and the irregularly mixed. This latter method has the further advantage of consuming less than 20 minutes of the subject's time.

On the whole, I think the results presented in this chapter demonstrate that the method of measuring attention proposed in these pages is a practicable one. The data of Tables XVI-XIX show conclusively that the detraction effect exerted by the unfavorable preparatory intervals, that is, by the irregularly mixed intervals, does not tend to wear off with a few hours practice. On the whole, the detraction effect is fairly constant from day to day. It is especially constant in subjects who are highly practiced in general reaction technique and yet in such subjects there is no evidence that the absolute size of the detraction is smaller than in unpracticed subjects. But even in unpracticed subjects there is no evidence that the detraction effect exerted by the unfavorable intervals wears off with practice. The six unpracticed subjects give results unequivocally against the conclusion that the absolute detraction effect decreases with practice. With subject St, Table XIX, it increases, and perhaps also with subject Lm, Table XVII. With a fifth subject, Sh, Table XVII, the results for the first 7 days are equally decisive against a decrease in the detraction effect of the unfavorable intervals as the result of practice. The results with the last part of the series of measurements made with this subject, however, as also the results with subject Sz, Table XVII, show certain peculiarities, due, as pointed out above, to marked lengthening as the result of practice, instead of shortening, in the reaction times with the 2 sec. intervals. These peculiarities require interpretation and the results may, therefore, be called inconclusive. The explanation that I have given of these peculiarities is that they are due to an error resulting from temporal adaptation. I can see no more evidence in the last mentioned results than in the others of a wearing off in the detraction effect exerted by the unfavorable preparatory intervals. The error of temporal adaptation is a serious one, but I have indicated how it may probably be avoided. Subjects Ll and Lm, given respectively 16 and 23 hours' practice, show no practice effect whatever in degree of attention.

In general, then, it may be said that the data of Tables XVI to XIX indicate clearly that the detractor I have used for measuring attention is one whose efficiency as a detractor is fairly constant and does not wear off with practice. The very fact of the general constancy in the effect of the detractor indicates that the degree of attention under the prevailing conditions did not improve. For the detractor itself remained the same throughout. This means, of course, that the attention which is involved in a simple reaction such as here used does not improve with practice.

If the degree of attention does not vary with practice, the question arises, how shall we account for the improvement in simple reaction time with practice. The answer is simply that the attention factor is only one factor in the reaction process, and that the improvement occurs in other factors. The equation[3] $y = A + B. \log x$ shows how this is possible. Whenever the degree of attention varies, the magnitude $B. \log x$ varies, and consequently it follows that as long as this magnitude remains constant the degree of attention remains constant. But y, the reaction time, may vary, as the equation shows, without variation in the magnitude $B \log x$, and therefore without variation in attention. Indeed, nothing is more certain than that reaction time may vary without corresponding variation in attention. Therefore, no new problem is raised when we find that reaction time improves with practice without any accompanying improvement in the degree of attention.

A much more important matter than the explanation of improvement in reaction time is the question of the rôle of attention in mental development in general—a most fundamental problem of the psychology of learning. It is not uncommon to explain improvement in any mental process as due to improvement in the degree of attention. And indeed, a large part of the apparent transfer of training from one process to another is often ascribed to improvement in attention. A few statements from the symposium on the doctrine of formal discipline by Angell, Pillsbury and Judd[4] will suffice to illustrate this widespread custom. Angell writes: "But, in any event, nothing is more certain than that a boy's auditory attention must itself receive separate training if it is ever to be of much value."[5] And in reply to the general question whether the serious pursuit of any study whatever may be expected to result in benefit for the subsequent pursuit of any other study, he answers that ". . . it seems probable that a certain gain in the power to use and sustain attention will accrue from any purposeful and persistent application." These results, he says, "may be expected to come in part from the suppressing or disregarding of disagreeable and distracting sensations, and in part from the discipline afforded to the common element in all acts of attention, whether this common element be found in some conditions of the cerebral cortex, or in some motor conditions which are essential concomitants of all attentive attitudes."[6] Thus, according to Angell, one of the fundamental causes of the transference of training lies in the improvement in attention which he assumes occurs with practice. Pillsbury, in explaining the effects of training on memory, writes that "In both rote and logical learning there are definite habits and capacities of attending to be acquired, and these may apparently be acquired in one field and used in another."[7]

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[3] See Summary, Chapter 1.

The above explanations are evidently invalid if it is true that attention does not improve with practice, a conclusion arrived at earlier in the chapter, so far as the attention involved in reaction is concerned. Since I admit that a more elaborate investigation of the effect of practice in attention may cause me to modify this conclusion, I shall not insist too strongly that the common view that attention improves with practice is erroneous. The experimental results I have already presented, however, seem to me clearly to indicate that the degree of attention involved in a given psychophysical process does not improve with practice, and that consequently *transference* of practice effects in attention is out of the question. As pointed out earlier in the chapter, the undoubted improvement in efficiency in a particular process is not a valid objection. What practice *may* do, is to change the conditions and the direction of attention. Better attention to something which we are more familiar with, more interested in, with more serious purpose, is not what I understand by improvement in attention. An improvement in the "capacity" of attention means that a greater degree of attention exists under certain given conditions than would have existed without that improvement. That the degree of attention changes when any of the conditions of attention are varied is not proof of improvement in the "capacity" of attention, unless the only condition varied is the number of times the attention has been exercised. Again, the "disregarding of disagreeable and distracting sensations" is no proof of improvement in the "capacity" of attention. On the other hand, the latter would be shown only by a heightened degree of attention to the main business in hand *without* lessened attention to the distracting sensations, or else increased attention to the latter without less attention to the former. Results presented in the latter part of this chapter on the distracting effect produced by an electric current show that this effect wears off with repetition, but this, I think, is due to a change in other factors than the degree of attention. While all such distractors are less attended to with practice, it is probably largely because of sensory and affective adaptation, so that these distractors can not be regarded as constant.[8]

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[4] *The Doctrine of Formal Discipline in the Light of Contemporary Psychology, Educational Review,* June, 1908.

[5] *Loc. cit.,* 11.

[6] *Loc. cit.,* 13.

[7] *Loc. cit.,* 27.

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[8] Pyle analyzes a case of learning to neglect distractions, and concludes as follows: "But a training with such results is really a training in habits of work, is a training in a certain response to a certain kind of situation, and in no proper sense a training of attention." *The Outlines of Educational Psychology,* 3rd ed., 1911.

While arguing against a true practice effect in the degree of attention, I wish at the same time to express the conviction that, contrary to general opinion, the question of whether or not a single faculty or capacity of attention exists, is still an open one. If it were possible to devise several widely different but definitely prescribed methods of measuring attention, making use of widely different types of mental processes, and if all individuals were found to have the same ranking by all of these methods, we should have proof of a general or single faculty of attention. This possibility can by no means be said to have as yet been disproved, if for no other reason than that the degree of attention of a number of individuals has not hitherto been measured by even one method in such a way that a ranking of their attentions is possible. The results of a study such as suggested in this paragraph will be presented in the near future. It is true that I have in the previous pages described two methods or submethods of measuring attention; namely, a method which uses sound as a stimulus and a method which uses light or a change in intensity of light as a stimulus. The data at hand do not permit me, however, to say whether the absolute effect of unfavorable intervals is the same in these two cases, or what degree of correlation, if any, exists between the results of the two methods.

Again, the conclusion that the degree of attention does not improve with practice should not be understood as denying a marked effect as the result of maturity. I have found such an effect, as is shown in the results of the latter part of this chapter. As long as a person is awake, attention is being practiced, and so the very existence of attention means its practice. But while attention must exist, in order that it may grow or mature, its existence, together with the practice inherent in its existence, is only a negative condition of its growth, not the cause of the growth.

(B) MISCELLANEOUS DETRACTORS

The following preliminary applications are given more for the purpose of illustrating the use of the method described in the preceding pages and leading to a better understanding of it than for the purpose of working out fully the problems to which the method is here applied. A general statement of the applicability of the method to the study of a wide range of conditions of attention has already been made in Chapter III.[9] It is evident that if we once have a satisfactory method of measuring attention we can measure the effect on attention of variation in any condition which can be varied, determining not only whether the condition varied is a condition of attention, but how the degree of attention varies with variation in the given condition. I have already presented several such studies in the preceding pages. In Chapter II, I have presented results on the effect of varying the objective condition of intensity, which show that intensity is a condition of attention, and in the earlier part of the present chapter, results on the effect of practice, which show that practice in a particular act of attention is not a condition of the degree of attention involved in that act. And in Chapter III, I have shown that variation in absolute intensity, within very wide limits, does not change the degree of attention to a constant relative change in intensity.

It would seem natural to expect that the degree of attention would increase with age. Careful observation indicates in many ways that an adult is capable of attending to a given task much better than a child. This seems so obvious that it may even be regarded as a test of the validity of any method of measuring attention that its application result in higher values of attention in the case of adults than in the case of children. I am unable at present to present elaborate data on the growth of attention. To do this it is necessary to measure the attention of a large number of children of each age, or else to measure the same children year after year from an early age up to maturity. The data here presented, however, are sufficient to show clearly that a great difference in attention exists between adults and children from 9 to 14 years of age. I have experimented on only three children, and a number of adults. Subject H. C. was a girl, 9 years, 0 months old, who had finished the A 3rd grade, and intended to enter the B 4th the following September. (The

[9] p. 71.

measurement here recorded was made in August.) Subject R. W. was a bright appearing boy of 14 years, 4 months, height, 5 ft. 3½ in., weight, 108 lbs. He had entered the first year of high school three months previous to the time of serving as subject in these experiments. Subject H. F. was a boy 10 years, 3, months old, weight, 63 lbs., height, 4 ft., 2 in., who had entered the B 4th grade three months previously. The attention of one of the children, H. C., was measured by the auditory method referred to in the preceding section of this chapter, results with which are presented in Table XVIII. With the other two children, R. W. and H. F., the visual method as described in connection with Table XVII was used. The results are presented in Table XX. For the sake of comparison I have given the range of final values obtained with all the adults measured by the same method as used with the children. By the final value is meant the average result of all the measurements made on a given individual, that is, the average of the differences obtained at each sitting between the reaction times with a regularly repeated 2 sec. interval and the reaction times with a set of totally irregular intervals. In the case of the adults this result alone is given, in the column headed 1/A. The headings of the columns have the same significance as in Tables XVI-XIX.

TABLE XX

Variation in Degree of Attention with Chronological Age.

Method	Subj.	Age	1st Series 2 Secs.		1st Series Irreg.		2d Series 2 Secs.		2d Series Irreg.		2 Secs. Av.	Ir-reg. Av.	1/A
			Av.	*M. V.*	Av.	*M. V.*	Av.	*M. V.*	Av.	*M. V.*			
Auditory	Adults	29	...	..	...	..	...	..	...	..	...	...	77
"	"	24	...	..	...	..	...	..	...	..	...	...	76
"	"	24	...	..	...	..	...	..	...	..	...	...	72
"	H. C.	9	271	*87*	495	*83*	227	*43*	510	*98*	249	503	254
Visual	Adults	29	...	..	...	..	...	..	...	..	...	...	83
"	"	28	...	..	...	..	...	..	...	..	...	...	101
"	"	27	...	..	...	..	...	..	...	..	...	...	67
"	"	26	...	..	...	..	...	..	...	..	...	...	66
"	"	25	...	..	...	..	...	..	...	..	...	...	85
"	"	25	...	..	...	..	...	..	...	..	...	...	62
"	R. W.	14	249	*17*	406	*62*	277	*41*	415	*76*	263	411	148
"	"	..	218	*18*	384	*57*	243	*31*	398	*46*	231	391	160
"	"	..	217	*23*	380	*55*	233	*29*	401	*70*	225	391	166
"	H. F.	10	378	*84*	536	*110*	326	*34*	654	*71*	352	595	243
"	"	..	282	*43*	508	*93*	364	*85*	653	*108*	323	581	258

In spite of the meager nature of the above results, the important conclusion may be drawn that a marked increase in degree of attention occurs with increase in maturity. While only three children have been used, the difference between the children and the adults is so immense that it leaves no room for doubt that the method used is plenty fine enough to bring out the effect of age upon attention in a striking manner. The results show that children are more affected by irregular intervals than are adults. The absolute increase in reaction time with the children is much greater in the case of reactions with irregular intervals than in the case of reactions with a regularly repeated 2 sec. interval. Of course, as should be understood by this time, the interpretation offered is that the irregular intervals constitute an unfavorable or detracting condition of attention, and that children are less able to resist the influence of detracting conditions than are adults; in other words, the children have a poorer attention.

As already stated there can be little doubt that there exists a great difference in the degree of attention of adults and that of children. One way to get different degrees of attention is to use subjects of different age. Now in Chapter III results were presented on the variation in the detraction effect produced by unfavorable preparatory intervals in the case of four different degrees of attention, the different degrees of attention being produced by the use of four different degrees of intensity of stimulus. In Table XX, also, we have results on the variation in the detraction effect of unfavorable preparatory intervals with variation in the degree of attention, the different degrees of attention being produced by the use of different degrees of maturity. The results in the present chapter lead to the same conclusion as those of Chapter III, namely, that the absolute detraction effect varies inversely with the degree of attention detracted from. The relative detraction effect, on the other hand, shows no great variation. The detraction effect exerted by the unfavorable preparatory intervals, then, is affected in the same manner by a decrease in the intensity of the reaction stimulus as by a decrease in the maturity of the subject. In both cases, the results show that it is the absolute detraction effect, and not the relative, which should be used in the measurement of attention.

In addition to the study of the effect of immaturity a study was made of the effect of a few other detractors, three of which (Nos. 1-3 below), were also distractors, resembling those commonly employed. The detractors used were the following:

1. An electric current, intermittent at the rate of 4 times a second, passed through the left hand.
2. Playing rather difficult music upon a German zither.
3. Putting blocks into a formboard with aid of vision.
4. One quart of Burgundy drunk about one-half hour previous to the reactions.

Only two subjects were used, one with detractor No. 1, and the other, with detractors Nos. 2-4. In the latter case, since the detractors required the use of the hands, at least Nos. 2 and 3, a Cattell voice-key was used for the reaction and the arrangement of apparatus was that described in connection with Table XV of Chapter IV, the size of the change in intensity being No. 4 of that table. With the electric detractor, however, the auditory method already referred to in connection with Table XVIII was used. In both cases the procedure was the same as used in the measurements of attention already described in connection with Tables XVII and XVIII,—first, 30 reactions with a 2 sec. interval, then 30 with a series of totally irregular intervals, a few minutes rest, and then 30 more with a 2 sec. interval and lastly 30 more with the set of irregular intervals.

The results are given in the following table, Table XXI, in which the column headings have the usual significance, except that in the column headed Conditions is given the detractor used, or else the fact that no detractor was used is indicated by the word Normal.

The results presented in Table XXI show that a number of ordinary detractors have a detraction effect upon the attention and they give the amount of this detraction effect. At the same time they show in a number of ways how unreliable such de-

TABLE XXI

The Effect of Various Detractors upon the Degree of Attention in Reaction.

Simple sound reactions used with subject Vs; visual recognition reactions with subject Ww.

		1st Series 2 Secs.		1st Series Irreg.		2d Series 2 Secs.		2d Series Irreg.		2 Secs.	Ir-reg.	
Subj.	Conditions	Av.	*M. V.*	Av.	*M. V.*	Av.	*M. V.*	Av.	*M. V.*	Av.	Av.	$^{1}/_{A}$
Vs	Normal....	142	*17*	199	*32*	117	*15*	211	*44*	130	205	**75**
“	Normal....	110	*17*	200	*43*	115	*11*	202	*31*	113	201	**88**
“	Current....	244	*96*	355	*105*	230	73	376	*74*	237	366	**129**
“	Current ...	166	*42*	343	*88*	235	77	308	*69*	201	326	**125**
“	Normal....	124	*12*	196	*25*	135	*12*	204	*26*	130	200	**70**
“	Current ...	168	*35*	285	*60*	185	*54*	294	*73*	177	290	**113**
“	Normal....	131	*17*	195	*24*	129	*18*	209	*25*	130	202	**72**
“	Current ...	184	*16*	275	*33*	183	*14*	264	*18*	184	270	**86**
“	Normal....	134	*12*	210	*22*	138	*16*	207	*27*	136	209	**73**
“	Current ...	177	*25*	258	*34*	173	*16*	281	*26*	175	270	**95**
“	Normal....	(Av	erag	e 5 se	ries)	...	..	...	..	128	203	**75**
“	Current ...	(	“	“	“)	...	..	...	..	195	304	**109**
Ww	Normal....	431	*50*	534	*48*	430	*36*	540	*54*	431	537	**106**
“	Normal....	421	*41*	530	33	415	*41*	497	*92*	418	514	**96**
“	Music.....	563	*50*	678	*64*	536	*49*	683	*71*	550	681	**131**
“	Form Board...	586	*55*	723	*70*	586	*61*	744	*83*	586	734	**148**
“	Burgundy .	453	*61*	621	*95*	500	*72*	684	*65*	477	653	**176**

tractors are for use in the measurement of attention. Of course, an electric current is not satisfactory, for however easy it may be to regulate physically, its effect upon the body cannot be accurately regulated, largely because of the great influence exerted by sensory adaptation. Adaptation is perhaps nowhere more marked than in the tactile sense, though clearly the difficulty applies in the case of many other detractors which one or another investigator has recommended, especially in the case of smell. In using the electric current as a detractor, I adjusted its strength at the beginning of each sitting so that it was approximately just not painful. An accurate comparison of the distraction effect produced on different days can not be made just because the effect of the current on the body could not be regulated, yet I believe it is a characteristic result I have obtained when I find that the detraction effect is 120 σ the first time the measurement is made but only 86 σ the fourth time. This is in keeping with the well-known tendency of the attention to be less and less diverted by such detractors. It is this fact, probably, more than any other, which has led to the conclusion that the degree of attention improves with practice, a conclusion which, as already pointed out, is of very doubtful validity.

Table XXI shows, moreover, that one can not predict whether such detractors as there used exert their effect wholly as detractors of attention, or whether they likewise affect the other factors than attention which are bound to be involved in any psychophysical act. For we find that the relation between the absolute prolongation beyond the 2 sec. reaction time produced by the detractor and the additional prolongation produced by the irregular intervals varies markedly in the case of different detractors. For instance, the alcoholic liquor produced only a slight increase in the 2 sec. reaction time but a very great decrease in attention, the latter point being shown by the great additional prolongation produced by the irregular intervals. The performance of music and the manipulation of the form-board blocks, on the other hand, produced a great prolongation in the 2 sec. reaction time, much greater than that produced by alcoholic liquor, but affected attention less than the liquor, since the irregular intervals did not produce such a great additional prolongation. Evidently, then, the performance upon the zither and the form-board were lengthening the 2 sec. reaction time by affecting in a detrimental way other factors than attention, or else the alcoholic beverage was affecting other factors than attention in a favorable way. The result shows clearly that we cannot be sure that any such detractors as these are purely detractors of attention, and accordingly that, apart from the difficulty and often impossibility of objective control, they are unfit for use as the detractor in the measurement of attention by the detraction method. The above results also demonstrate conclusively that simple reaction time does not in all cases vary directly inversely with the degree of attention. That it does so under constant conditions is the basis of the present work, but equally basic is the fact that it does not do so under varying conditions and consequently, therefore, could not be used as a measure of attention under varying conditions. This means that simple reaction time is practically worthless as a measure of attention. It varies with attention, but with many other factors as well. By means of simple reaction time, or indeed by means of the simple measurement of efficiency of any sort, we could compare neither different individuals nor the same individual on different days, or even at different times of the same day under otherwise apparently constant conditions, for the physiological condition varies from day to day, and at different times of the day, and without a measure such as described in the present treatise, we should have no way of being sure whether the variation in reaction time were due to variation in degree of attention or to variation in other factors. To investigate by means of simple reaction time or any other simple efficiency the effect upon attention of any conditions which affect anything else than attention, would be out of the question, and in all such cases we should have to remain ignorant of whether the condition did affect other factors than attention. On the other hand, by the use of a detraction method such as I have described in the preceding pages, which employs a detractor which detracts solely by acting upon attention, the influence upon attention of any variable condition can be readily ascertained; and further, the effect of variation in this condition upon the efficiency of the act in which the effect of the variation becomes noticeable can be analyzed into an effect upon attention and an effect upon other factors than attention, and the magnitude of each of these two kinds of effects may be determined separately.

SUMMARY

The method, described in the present chapter, for the measurement of the degree of attention is capable of being applied over and over again without any general tendency for the values obtained for the degree of attention to either increase or decrease. In highly practiced subjects, the irregular intervals continue to constitute a marked and very constant detractor. In four out of six subjects who were altogether unpracticed in reaction time experiments at the beginning of the work reported in this chapter there was no evidence of the slightest tendency for the magnitude of the detraction effect produced by the irregular intervals to decrease with practice. In one of these four, the detraction effect increased slightly towards the end of a series of ten measurements. In the case of the other two of the six unpracticed subjects a peculiar increase with practice was noted in the reaction time with the regularly repeated 2 sec. intervals, and this increase produced an apparent decrease in the magnitude of the detraction effect. This peculiar increase with practice in the reaction time with the 2 sec. intervals may be explained as due to the phenomenon of temporal adaptation. This phenomenon consists in an adaptation on the part of the subject to the irregularly mixed series of preparatory intervals of 4 to 20 secs., an adaptation which normally occurs in working with such a series of intervals and which I have described in Chapter II, but which in the present instance carried over to the work with the regularly repeated 2 sec. intervals and there exerted a detrimental influence by checking the natural tendency of the subject to adapt his attention as quickly as possible and so attain a maximum adaptation at the end of 2 secs. This phenomenon of temporal adaptation may, therefore, be regarded as a source of error in the measurement of the detraction effect exerted by the unfavorable intervals, but I have described a procedure whereby this error may probably be avoided, and was avoided in the case of the subjects with which it was used (the subjects in Tables XVI and XIX). It should be noted that this phenomenon of temporal adaptation never comes in as a source of error except in a *series* of measurements. If this phenomenon of temporal adaptation is regarded as the explanation of the decrease found in the case of two of the subjects in the detraction effect produced by the irregular intervals, it may be said that the data of this chapter afford very nearly conclusive evidence that the degree of attention involved in the reactions shows no general tendency either to decrease of to increase with practice. The data show that in the method here described we have a method of measurement that can be applied a number of times without much variation in the result. The degree of practice the subject has had does not need to be taken into consideration.

Since the degree of attention as involved in the reaction experiment does not improve with practice, in spite of the fact that reaction time itself may shorten considerably with practice, I am inclined to believe that it is generally true that the degree of attention involved in any specific psychophysical process will show no improvement with repetition of that process. Improvement is here understood to mean an increase in the degree of attention while all the conditions of attention remain constant,—aside from the number of times the specific psychophysical process in which attention is involved has occurred. In other words, the mere repetition of an act, even with the purpose in mind at each occurrence of the act to be as efficient as possible, does not bring about any result which increases the degree of attention involved in the act. It may seem to, but only because other factors, which can be kept constant under proper experimental conditions, have not been kept constant. If the degree of attention in a particular psychophysical process does not improve with practice, it follows, of course, that "transference of training" cannot be explained even in part as due to improvement in degree of attention. At the same time, it seems to me that the existence of a single faculty or single general capacity of attention must be admitted to be still an open question awaiting an experimental decision.

Application of the method described in the preceding pages to the study of miscellaneous detracting conditions gives results which corroborate the validity of the method. Such detracting conditions as rapid electric shocks, the performance on a musical instrument, or fitting blocks into a form-board, simultaneously with reacting, and the presence of alcoholic liquor inside the body resulted in a marked lowering in the value obtained for degree of attention. Analysis of the results shows at the same time that simple reaction time is entirely unsatisfactory as a measure of attention, and also that such detractors as the above would be altogether inaccurate as detractors to be used in the measurement of attention by means of the detraction procedure; that is, they could not be used in the way I have used irregular preparatory intervals.

The results of Table XX show that the degree of attention of adults is very much superior to that of children. Further, by assuming *à priori* that attention increases with maturity, the results of Table XX lead to the same conclusion as those of Chapter III, namely, that the absolute detraction effect exerted by unfavorable preparatory intervals varies inversely with the degree of attention detracted from. This same conclusion was also reached in Chapter IV from the results of Table XV. The results of Chapter III and those of Table XX, taken together, show that the detraction effect produced by the unfavorable preparatory intervals is affected in the same manner by a decrease in the intensity of the reaction stimulus as by a decrease in the maturity of the subject. The results in both cases make it clear that only the absolute detraction effect of the unfavorable preparatory intervals, and not the relative, can be used as a measure of attention.

CHAPTER VI

General Summary

It is not my intention to restate here all the conclusions that may be drawn from the experimental work reported in the preceding pages. The most important of these conclusions have already been summarized at the end of each chapter. In conclusion, however, I shall give a general account of the method for measuring attention that has been worked out in the preceding pages. This account may, perhaps, best be made by basing it upon an outline of the distraction method, inasmuch as the distraction method is one with which every psychologist is familiar, and one that has been generally regarded as one of the most hopeful of the methods by which the measurement of attention has hitherto been attempted.

There are a number of ways in which the distraction method may be formulated, but the following schematic statement will, I think, be found fairly just. First, the efficiency of the subject in some work is to be determined under maximally favorable conditions. This work may be any kind of work which requires attention, as, for instance, the cancellation of certain letters of a printed page or the discrimination of a small difference in brightness. Second, the efficiency of the subject in this work is to be determined while he is working under definitely specified distracting conditions, or distractors, all other conditions remaining the same. A distractor is some condition which takes away a part of the subject's attention from the main task in hand. For instance, if a subject were to cancel the *A*'s in a page of printing and at the same time to count the beats of a metronome, the counting of the metronome beats would constitute the distractor. Or, in case the subject were not required to count the metronome beats, then merely the sound produced by these beats might be called the distractor. After the decrease in efficiency resulting from a given distractor has been determined, this decrease is somehow to be utilized as a measure of attention, presumably on the assumption that the decrease in efficiency produced by a given distractor varies inversely as the degree of attention against which the distractor acts. In short, attention is to be measured by measuring the ability of the subject to resist distraction.

I believe the general principles of the distraction method are sound. Practically, however, this method has been a failure. One of the chief reasons for this failure is that it has been found impossible to obtain a satisfactory distractor. A satisfactory distractor is one that, among other things, is constant or uniform in its effect. It must not greatly decrease the subject's efficiency at one time, and then hardly affect it at all at another time. No distractor which is constant in its effect has yet been discovered. Those so far used are not only irregular in their action but, for the most part, soon lose their efficiency.

One of the chief reasons for the failure of distractors is that they require divided attention, though this division may, perhaps, be thought of rather as an oscillation of attention. When the subject's attention is divided, it is impossible for the experimenter to control the proportion between the two parts. I have, therefore, undertaken to work out a method which would preserve the fundamental principle of the distraction method, but would not involve divided attention upon the part of the subject. The fundamental principle of the distraction method is not to distract, that is, to rend attention asunder, but merely to increase the difficulty of attention, to introduce a definite resistance. Now it is possible to increase the difficulty of attention without asking the subject to divide his attention. An important paragraph in any textbook of psychology is entitled the *Conditions of Attention.* Under this heading we have given a number of conditions which bring about a high degree of attention. Many, and probably all, of these conditions may be varied, that is, they are subject to quantitative gradation. Take for instance the so-called objective condition, intensity of stimulus. The intensity of the stimulus may be increased or decreased. And when we speak of intensity of stimulus as a condition of attention, what we mean is that the more intense the stimulus, the more attention it attracts. Other things equal, a bright light attracts more attention than a dim one, and a loud sound attracts more attention than a soft one. Evidently, then, by weakening the intensity of a stimulus, we make a change which is unfavorable to a maximal degree of attention.

There are many other conditions of attention than intensity. An unfavorable state of any of these conditions is unfavorable to the highest degree of attention, but does not necessarily result in division of attention, and, therefore, cannot be spoken of as a distractor. It is proposed to use the term *detractor* to designate an unfavorable state of any of the conditions of attention. A distractor tears attention apart, a detractor merely reduces the degree of attention—no matter how. A detraction method of measuring attention, then, is the same as a distraction method, except that the distractor of the distraction mehod is replaced by a detractor, i.e., an unfavorable state of one of the conditions of attention, and replaced by a detractor which does not require division of attention.

In the detraction method described in the preceding pages the act the efficiency of which is measured is the act of reaction. When the subject is instructed to react as quickly as possible, the subject's efficiency is measured by the reaction time. In reaction time work it has always been customary to have a warning signal precede the stimulus by an interval of about 2 secs. This warning signal is given in order to allow the subject time to get ready to react as quickly as possible, so that the interval between the signal and the stimulus is called the preparatory interval. With a preparatory interval of 2 secs., especially after it has been repeated a few times, the subject knows exactly when to expect the stimulus, and so reacts with maximal adaptation of attention. Now when the procedure is varied so that the subject does not know just when to expect the stimulus the reaction time is much longer. For instance, if the reaction time to a visual stimulus with a regularly repeated 2 sec. preparatory interval is 200 σ, with a set of intervals varying from 4 to 20 secs. in length, given in entirely irregular order, it may be, perhaps, 300σ. In the case of the reaction experiment, therefore, one of the conditions of maximal attention is a preparatory interval of 2 secs. Longer and irregularly mixed intervals on the other hand bring about a lower degree of attention. They decrease the degree of attention under which the reaction occurs, so that as regards the attention involved in a reaction, they constitute a detractor. The method, then, stated briefly, consists in determining first an individual's reaction time with a regularly repeated 2 sec. preparatory interval, and second, the prolongation produced in this time by substituting for the regularly repeated 2 sec. interval a set of irregularly mixed intervals of widely different length. This prolongation remains, on the average, constant in the case of a given individual, and does not decrease with moderate practice. This fact not only demonstrates the constancy of the action of the unfavorable preparatory intervals as detractors of attention, but means that the measurement of attention by this method does not vary with practice. This latter fact differentiates this measurement from most, if indeed not all, mental measurements so far in use.

A question of fundamental importance which remains to be answered is, just how is the prolongation produced by unfavorable preparatory intervals to be used as a measure of attention? Shall we, for instance, use the reciprocal of the absolute prolongation or the reciprocal of the relative prolongation? This problem was solved by artificially producing different degrees of attention, and applying the detractor to these different degrees of attention. In order to obtain the different degrees of attention, among other methods, use was made of one of the conditions of attention already referred to, namely, intensity of stimulus. Four different intensities of stimulus were used, on the assumption that the weaker the intensity the lower would be the degree of attention. It is true that weakening the intensity of stimulus brings about a lengthening of reaction time as the result of its effect upon other factors involved in the reaction than attention. It is not a pure detractor of attention. This fact would clearly prevent the use of weak intensity as a detractor for the purpose of measuring attention by a detraction method. It is largely this necessity of having a pure detractor that led to the selection of unfavorable preparatory intervals. Unfavorable preparatory intervals exert their detrimental effect upon reaction time solely by bringing about a lesser degree of adaptation of attention at the instant of the reaction, whereas weak intensity of stimulus slows reaction time not only by lessening the degree of attention, but in other ways as well, e.g., lengthening the latent period of retinal stimulation. Nevertheless, intensity is a condition of attention, so that the use of four different degrees of intensity of stimulus brings about four different degrees of attention. Having thus secured four different degrees of attention, I then applied my detractor, namely, the unfavorable preparatory intervals, to each of these degrees of attention, and found that the absolute prolongation in reaction time was different in each case, being greatest in the case of the lowest degree of attention (with the weakest degree of intensity) and least in the case of the highest degree of attention (with the strongest degree of intensity). The relative prolongation, on the other hand, tended to a certain extent to remain constant. It follows that in the measurement of attention we should use the absolute decrease in efficiency produced by the detractor and not the relative; that is, we should make our measurement on the basis of the following law of detraction, namely, that *the absolute detraction effect produced by a given detractor varies inversely as the degree of attention detracted from.* Three independent demonstrations of this law are given, one in each of Chapters III, IV andV. The degree of attention is measured by the reciprocal of the absolute detraction effect.

It is often desirable to measure the degree of attention apart from the condition of sensory sensitivity. I believe, on the basis of the results presented in Chapter IV, that this can be accomplished, at least with the exception of extreme cases of sensory insensitivity, by using as the reaction stimulus a supraliminal change in intensity. This can be done most easily, probably, by using light as the stimulus.

To sum up, the degree of attention involved in reacting may be measured, in the sense of ranked, as follows: by obtaining, first, the average reaction time of 30 reactions, using a regularly repeated preparatory interval of 2 secs.; second, the average reaction time of 30 reactions with a set of irregularly mixed preparatory intervals of widely different length; and third, equating the degree of attention with the reciprocal of the absolute difference between the two aforementioned average reaction times. It is highly advisable, in case a long series of measurements is intended, not to make more than one measurement a day, as otherwise, after a few measurements, the phenomenon of temporal adaptation may come in as a disturbing factor. Further, it is of course essential that no condition other than the preparatory interval should be changed at any time during the course of a single measurement. The "motor" or "abbreviated" form of reaction should be used, that is, the subject should always be instructed to react as quickly as possible. As the reaction stimulus, I regard a large and sudden increase in brightness as the best, and as the reaction movement, I prefer the removal of two fingers of the right hand from a reaction key, but both the reaction stimulus and the reaction movement may be varied to suit the purpose of the experimentation. A choice or discrimination reaction may be used whenever there is any danger that the results may be influenced by a tendency to premature reactions.

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